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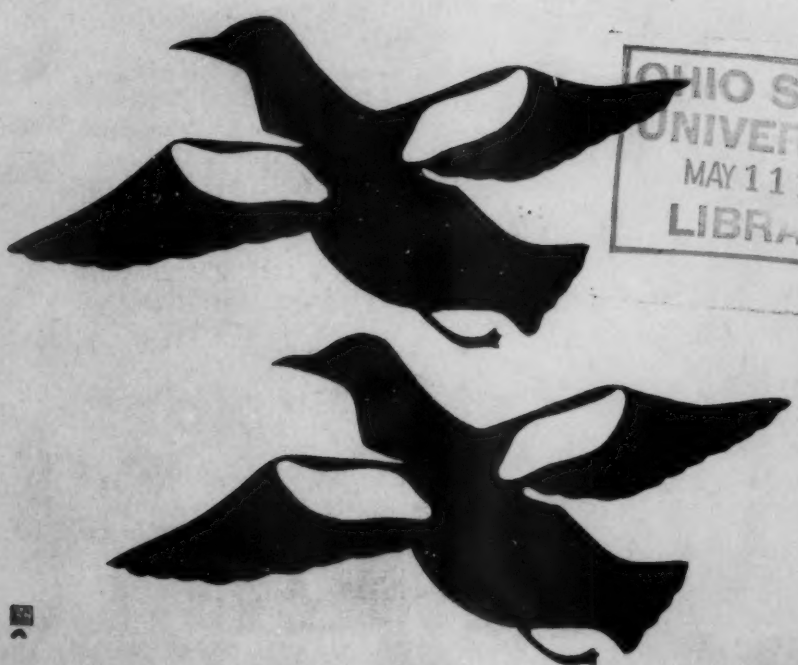

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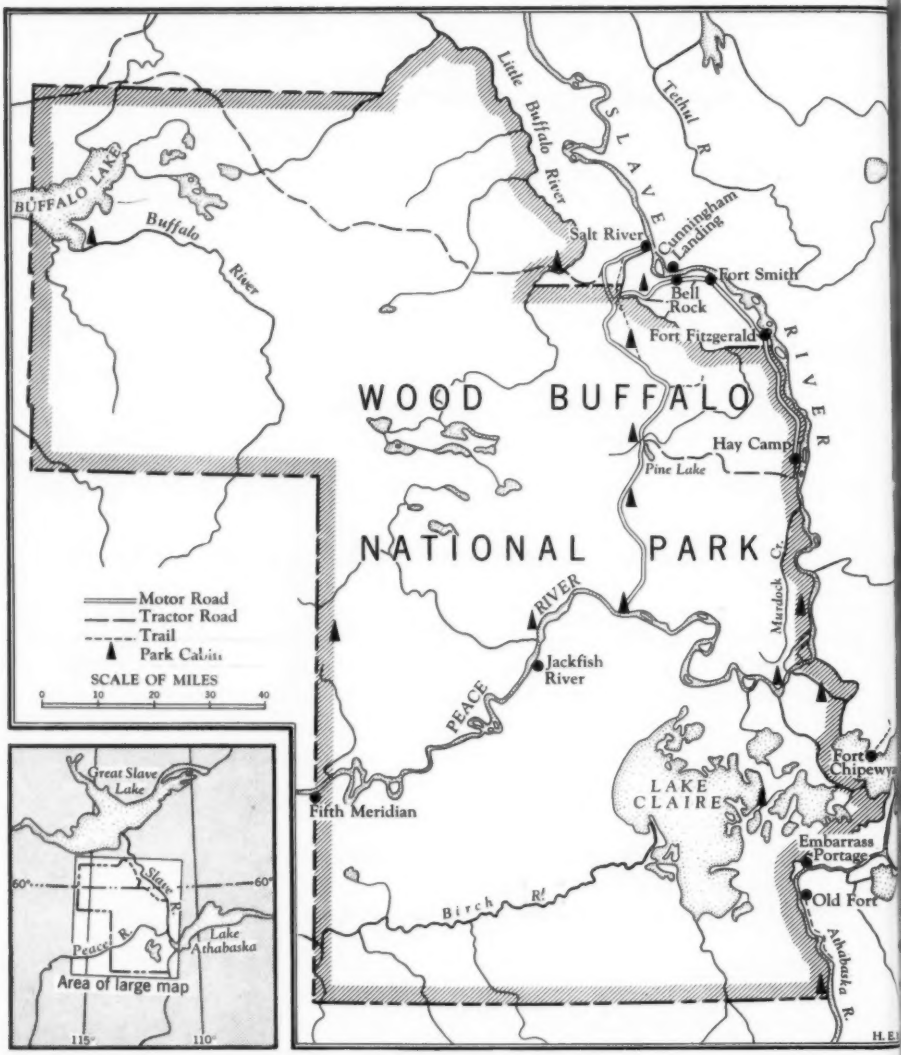

Fig. 1. Map of Wood Buffalo National Park.

BEHAVIOUR AND SOCIAL ORGANIZATION OF THE WILD BISON OF WOOD BUFFALO NATIONAL PARK, CANADA

W. A. Fuller*

FROM 1950 to 1956 I was engaged in a study of the biology of the bison in Wood Buffalo National Park with particular reference to the effects of tuberculosis. During this time I had many opportunities to observe their behaviour. The purpose of this report is to record my observations.

Wood Buffalo National Park (Fig. 1) was originally set aside in 1922 for the protection of the last remnant of the wood bison *(Bison bison athabascae)*. However, in the years 1925 to 1928 inclusive, more than 6,000 plains bison *(B. b. bison)* were introduced into the park where they interbred with the estimated 1,500 remaining wood bison. The population now in the park is considered to consist almost entirely of a mixture of the two races. Animals closer to the plains type predominate. The animals are completely in the wild state. A few hundred are rounded up annually and held in corrals for a few weeks before slaughter. There are no other fences, and supplementary feeding is neither required nor practised. All observations refer to free-ranging bison unless the corrals are specifically mentioned.

I. Behaviour

Roe (1951) states that much of the evidence concerning the habits of the bison is contradictory. He documents this by extensive quotations from earlier literature in his chapters five, six and seven. The contradictions are readily understood if one remembers that the plainsmen of the nineteenth century were not, for the most part, trained and critical observers. Even Hornaday (1889), one of the few trained zoologists to study the bison during this period, was guilty of accepting dubious evidence at its face value and thus helping to perpetuate a number of fallacious beliefs. In a later book (1922), he showed himself to be a close observer of certain aspects of the behaviour of captive bison. Similarly, Garretson (1938), whose book is weak in many respects, was surprisingly accurate in his brief descriptions of behaviour. Seton (1929), although generally accurate, has uncritically quoted some doubtful statements of earlier writers, thereby implying his acceptance of their beliefs.

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Two investigators of the northern bison have published reports that dealt in part with behaviour and social organization. Goodwin (1935, 1939) wrote semi-popular accounts of the Snyder expedition, whose chief purpose was to secure museum specimens. Soper (1941) published a detailed account of his two-year study of the bison including many references to behaviour. The most complete description of bison behaviour is the recent work of McHugh (1958).

Characteristics of bison that influence behaviour

Senses. The capacity of an animal to adjust to its environment, either physical, biological, or social, depends in large measure upon the possession and relative development of sense organs. Those forms with well developed senses are better equipped to detect changes in the environment (stimuli) and make satisfactory adjustments (behaviour). In general, the senses of the bison are well-developed. McHugh (1958) found that bison detected a moving jeep at distances of more than a mile and a horse and rider at 0.8 mile. I have had similar experiences on foot and with motor vehicles of various types. I have noticed that the flight distance is usually greater from a man on foot than from a motor vehicle. It is apparent, therefore, that bison can see, and distinguish between, objects at considerable distances.

As might be expected from the prominent external ear the sense of hearing is also acute in bison. They react to small noises such as the accidental cracking of a branch underfoot at distances of 100 to 200 yards. On cold winter days the noise of frozen branches brushing against heavy clothing can also be detected by the animals for some distance and this will often spoil a stalk. On one occasion I was watching a herd of bison and noticed that they began to focus their attention in my direction, but were looking beyond my hiding place. After some time I heard bison vocalizing and later a second herd appeared from the surrounding woods. Since there was a cross wind it is evident that the first herd initially detected the presence of the second by auditory stimuli. It is equally apparent that the bison reacted to the sounds of the second herd long before those sounds were audible to me.

The sense of smell is highly developed and appears to be of prime importance in detecting danger. Bison will frequently permit a guarded approach by a person that they have detected either by sight or hearing, but I cannot recall an instance of hesitation once they had caught my scent. On this point McHugh (1958) stated "Whenever scenting humans the Hayden Herd stampeded without hesitation even though the source of scent was not visible". I have seen many striking examples of this. On one occasion, after watching a herd from a place of concealment for nearly an hour, a sudden shift in the wind, which carried my scent to the bison, precipitated an immediate stampede. I have no quantitative data on the maximum distance at which bison can detect human scent, but it is at least several hundred yards.

The chemical senses are also used by males to detect a female in heat. They may also be important in individual recognition, for example the identification of a calf by a cow following temporary separation. I have even seen two adult males approach and smell each other in a manner reminiscent of dogs.

The sense of touch seems to be of minor importance in bison behaviour.

Fig. 2. Bison cow swimming in Lake Claire.

Locomotion. Bison, in spite of their ungainly appearance, are swift runners. When "pushed" from behind by a truck with a horn blaring and people shouting they are capable of a top speed of between 30 and 35 miles per hour and they can maintain such an all-out effort for up to half a mile. There seems to be no significant difference between the top speed of males, females, and calves only a few weeks old. Even calves so young that a few inches of the umbilical cord are still visible, can keep up with the herd for short distances.

Bison are also powerful swimmers. When swimming the hind quarters are low in the water and only the head and top of the hump show. I observed a lone cow swimming strongly from the mainland to an island in Lake Claire, a distance of about a mile and a half (Fig. 2). I followed her in a canoe and noted that she showed no undue signs of fatigue on landing. Bison also regularly swim the Peace and Slave rivers, which are approximately half a mile across and have currents running from 2 to 4 miles per hour.

Vocalization. Bison are capable of producing a number of sounds. The "bellow" is the one best known. It is heard most frequently as the rutting season approaches and seems to be a manifestation of the increasing restlessness of the males. Two other sounds are made frequently by males and less frequently by females. The "snort" can be imitated by forced inhalation with fluttering of the soft palate and the "cough" by forced exhalation past a glottal stoppage. "Snorts" and "coughs" are associated with agonistic (fighting) behaviour. I have rarely heard any of the grunting sounds described by McHugh (1958) except from animals being herded around in the corral.

Cows searching for lost calves have been heard to utter a series of snorts. Calves have not been heard to respond in the wild, but in the corral they do a good deal of bawling.

Anatomical peculiarities. Two structures possessed by bison play a prominent part in certain types of behaviour. The horns, efficient weapons for fighting, are prominent in agonistic behaviour. They are also used to dig or at least scratch the surface of wallows (grooming behaviour) and in horning trees (possibly a type of "displacement activity").

In this region, with its hordes of biting insects, the importance of the tail as a fly switch is obvious. The tail also functions less obviously as an indicator of the emotional state of the animal. In any situation involving the attack-escape drive the tail is held erect. Males showing investigative sexual behaviour also usually carry the tail erect or partly erect.

Social behaviour

Scott (1958) classifies all behaviour under nine headings. I have data that have a bearing on social behaviour under five of these headings.

Epimeletic (care-giving) behaviour. Adult males indulge in a considerable amount of grooming particularly in the summer months. Ordinary grooming consists of rubbing against stumps, trees, or rocks, and wallowing. These mannerisms are well known and have been described by Soper (1941) and McHugh (1958). I observed one instance of what I interpreted as mutual grooming of two adult males. These two exchanged gentle horn digs in the thorax and abdomen. This exchange was not accompanied by any aggressive actions. The result appeared to be mutually pleasurable and may have been similar to the sensation achieved by rubbing against trees or rocks.

Females do much less wallowing and rubbing than males. Their chief expression of care-giving behaviour is through nursing the young. Although I found solid food in the stomachs of calves shot in July for disease studies, weaning seems to be a protracted affair. The udders of cows shot in January contained copious amounts of milk and milk could be expressed from the nipples by hand.

Females are also concerned with the defence of the calves, both from natural predators — wolves — and from humans. I recorded one instance

of a cow remaining with a calf crippled by wolves. My fortuitous appearance frightened away the wolves (of which I glimpsed two) and started a stampede among the bison. One cow remained behind, and when I sought a reason for her refusal to leave, I found the nearly dead calf lying behind a fallen tree. On one occasion I was threatened and driven off by a cow when I attempted to photograph and capture a young calf that had become exhausted during a chase by motor vehicle. Another time, after I had shot a calf for a *post mortem* examination a cow would not let me approach the carcass. After several attempts to do so had failed the cow finally had to be shot.

Seton (1929), Garretson (1938) and Soper (1941) have all pictured the bulls as defenders of the young. I have seen no indication of this in Wood Buffalo Park, nor did McHugh (1958) in the herds he studied.

Allelomimetic (contagious) behaviour. Allelomimetic behaviour occurs in two important situations — in the disorderly type of escape known as a "stampede", and in winter when a herd must travel through snow. The question of leadership is closely related to this form of behaviour. Soper (1941) and Seibert (1925) attributed the position of herd leader to a "majestic bull" whereas McHugh (1958) thought that "cow groups" were most frequently led by older cows. My observations suggest that in mixed groups the situation determines the leader in no small degree.

When breaking trail through new snow a bull (or bulls if more than one is present) is usually in the van and the weaker animals string out behind. If the group is small the animals proceed in single file whereas larger groups may proceed two or more abreast. The adaptive value of this arrangement is obvious.

Two patterns of flight behaviour have also been noted. Usually when the herd is approached in the open I agree with McHugh that the older cows are the most alert members of the group and are likely to take the lead in the ensuing "stampede". I noted a different reaction, however, when groups were surprised on the twisting fire trails through the park. Frequently the herd retreated down the road and disappeared from view around a bend or over a slight rise. Once out of sight of the source of danger the herd paused. If the observer followed along the road, he would see first a mature bull facing in his direction. Fifty to one hundred yards down the trail the rest of the herd would be closely bunched, usually facing away from the observer, but looking back over their shoulders. At the first appearance of the observer, the "rear-guard" bull would wheel and run in the direction of the herd. The wheeling of the bull appeared to be the signal for the herd to retire. A herd disturbed a second time in this manner rarely paused again until it had travelled some distance into the shelter of the forest. The pause after the first short flight seemed to serve the purpose of confirming that a real source of danger existed. The bull assumes the function of a leader in this case even though he is bringing up the rear.

On this point Soper (1941) says "he [the bull] is more often the one to leave last as the animals take fright and dash away together into the woods". The only other mention of a similar behaviour that I have seen is by Grinnell (1904) who thought that the bulls brought up the rear simply because they were slower, and not from any "desire" to protect the herd.

Allelomimetic behaviour is prominent in the play activities of calves. Games of "follow the leader" and sudden sprints and chases are common forms of play.

Agonistic behaviour. An aggressive attitude is displayed chiefly by posture. Vocalization ("coughs" and "snorts") accompanies the threat posture in a minority of cases. A mild threat is conveyed by the rigidly erect tail. With increasing stimulation of the attack-escape drive the head is lowered and, usually, swung slowly from side to side. Finally, the threatening bull, with tail up and head down, paws the ground with a fore foot.

There is lack of unanimity among observers as to whether or not fighting bulls charge one another. Goodwin (1935), Cahalane (1947), and McHugh (1958) all mention charges. My observations, however, agree with Garretson (1938) and Hornaday (1922) who state that the animals advance slowly. In fact, the actual encounters that I saw had a stylized appearance. Heads were lowered and carried slightly to one side; the animals advanced and locked one horn; there was some minor shifting as though to find a comfortable position for the locked horns, and the battle commenced. There resulted a shoving match, with the object apparently of turning the opponent broadside in order to deliver a telling blow. Should the contact of the horns be broken, it was carefully rejoined by the method described. The longest battle I witnessed lasted approximately 5 minutes during which time the larger of the two combatants pushed the smaller across a road, down through a 4-foot ditch and about 30 yards across a prairie. The smaller animal was holding its own on level ground when the encounter came to a sudden and (to me) unexpected end.

Aggressions among males, are not restricted to the breeding season, but seem to increase in frequency at this time. Soper (1941) stated that there were "many battles" during the rut, but my observations fail to substantiate this. Much depends on the interpretation of the word "many". In my experience most encounters between bulls are decided by threats (McHugh's "passive dominance") and never reach the fighting stage. This implies that dominance relations are well defined. Wardens and other casual observers frequently described to me vicious battles they had seen. It is only natural that they should be impressed by the occasional fight, but not by the more frequent encounters wherein no fight ensued. Soper depended on the testimony of wardens to support many of his observations, and this may have led him to overemphasize the occurrence of battles during the rut.

One other situation seemed to stimulate attack and escape drives simultaneously — the sudden intrusion of a loud noise. Nearly always

when "buzzed" by a low-flying plane any bulls that happened to be close to each other engaged in battles, lasting only a few seconds, before turning and fleeing. The same behaviour was frequently observed in groups of bison standing on the road when I came suddenly on them in a noisy truck (a Power Wagon). I observed it once in a group standing near the bank of a narrow river when I rounded a bend in a canoe propelled by an outboard motor.

Aggressions between females were not infrequent but they occurred so unexpectedly (i.e. without preliminaries) and were so quick and brief that I was unable to note details.

Sexual behaviour. The earliest indication of the onset of the rut is an increased restlessness in the bulls; this is shown in two ways — first by uprooting small trees, generally spruce seedlings 3 to 5 feet high, and second by increased vocalization. Both these activities take place mainly at night. By day there is little or no change in the tenor of the herds likely to be noticed by the casual observer. The preliminaries to mating are only seen if one watches an undisturbed group for some time.

The sexual actions of the males later in the rut are quite characteristic. A male approaches a female with his neck lowered, head extended, lip upcurled, tail partly or fully erect, and smells her external genitalia or freshly voided urine. A bull may investigate several females in this manner in a short space of time or he may stay close to one cow. McHugh (1958) called this latter type of association a "tending bond". He found, as I did, that most tending pairs took up a position near the periphery of the group. In this study, and in McHugh's, only fully mature bulls were observed to tend a cow. McHugh also recorded several examples of atypical tending bonds. While I did not observe anything of this nature, I do not consider my observations to be extensive enough to rule out the possibility of atypical tending bonds occurring in the northern bison.

I have seen a tending bull direct a cow by placing his extended chin against her head and gently forcing her to turn through 90 degrees. He then resumed his place at her side and began licking her flank. On another occasion, a bull "nuzzled" a cow in the neck, forcing her to turn her head, whereupon he began to lick her cheeks and chin. These actions could be described as "amatory" behaviour.

I observed many attempted mountings but did not see any complete copulations.

Seton (1929) has labelled the type of mating of the bison polygamous (more precisely, polygynous), and with this most modern writers have agreed. The herds, at the time of the rut, contain many more mature females than males, an observation that seems to support this thesis. However, I have shown (Fuller 1957) that by no means all cows conceive each year — only a little more than half the cows in the Hay Camp herds, which have been studied most intensively. The ratio of males to receptive cows, therefore, is much closer to equality, so that the sex ratio of a herd, in itself, proves nothing. Pre-copulatory behaviour is also inconclusive, since

bulls have been observed to tend several cows in rapid succession. The question can only be definitely decided by observing how many cows are served by one bull. I have had no opportunity to make these observations, perhaps because, as suggested by several writers (e.g., Cahalane, 1944), mounting occurs most frequently at night. McHugh (1958) classed the type of mating in a confined herd as temporary monogamous. He thinks the bulls are promiscuous, but that a given cow is served by only one bull.

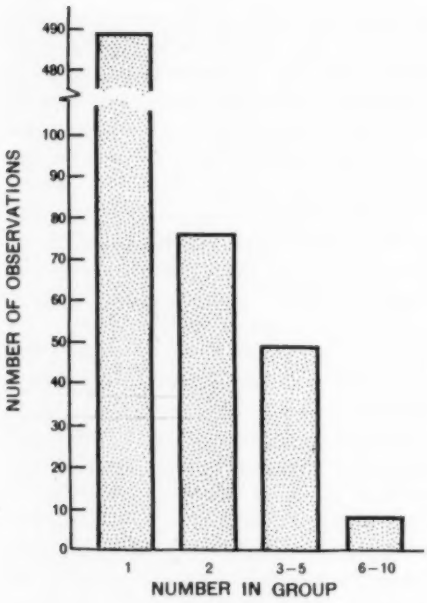

Fig. 3. Frequency of occurrence of groups of male bison in forested habitats.

II. Social organization

Herd composition

McHugh (1958) classified buffalo into bull groups and cow groups according to composition. I prefer the term "mixed group" to "cow group" for the northern bison because these herds nearly always contain some mature males as well as mature females and young of both sexes.

My observations on groupings were made mainly in the summer season from June to September because it was only at that season that the animals could be conveniently studied on the ground. Observations made from planes at other seasons of the year, however, indicated that the bull groups and mixed groups remained apart throughout the year.

Bull groups. In forested habitats the bull groups I observed never contained more than ten individuals. Road counts in the forested habitats from 1950 to 1953 inclusive yielded 488 lone bulls, 88 groups of two, 49 groups of three to five, and only 9 groups of six to ten (Fig. 3). Bull groups tended to be larger on the meadows bordering Lake Claire. The largest bull group I observed was seen there from a plane and contained 30 members. Groups of 15 to 20 are relatively common. McHugh (1958) found the bull groups in Yellowstone Park to be composed of one to twelve members.

The totals presented are biased by an unknown amount of duplication. The sandy roads seemed to attract the bulls, probably because of the ease with which dust wallows could be made in them. The ditches and spoil-banks were also much used by the bulls for resting areas. Thus the bull groups tended to stay on or near the roads and the same groups were probably tallied several times. For example, a lone bob-tailed male, probably the same individual in each case, was observed at approximately the same place six times in the month of August, 1954.

Old age was not the rule among the lone bulls. Approximate ages, based on horn wear, (Fuller 1959), were assigned to 370 of the 488 observed. Of these, 34 per cent were 7 years or older, 65 per cent were 4 to 6 years old (i.e. just reaching full maturity) and the remaining 1 per cent were 2 and 3 year-old spike-horns. These figures contradict the thesis put forward by many writers, including Goodwin (1939) and Soper (1941) for the northern bison that the lone bulls are old animals driven from the herd by younger mature bulls.

Spike-horn males (2 and 3 year-olds) either remained in the mixed herds or were associated with the larger bull groups. They were seldom found alone (see above) or as members of a pair. These observations agree with McHugh's (1958) for Yellowstone Park buffalo.

Mixed groups. Age and sex counts were made for the most part along the narrow roads in forested terrain. Some members of a herd frequently remained among the trees where they could not be identified as to age and sex, and therefore the counts were seldom complete. The most nearly complete segregated counts obtained during the summers of 1950 and 1953 are shown in Table 1. No winter counts were made.

Slightly over 86 per cent of the animals in these counts were classified as to age and sex. To disregard the remaining 13.6 per cent would introduce a bias because it is known that they were mostly mature females and immatures of both sexes — the groups most difficult to separate. Therefore, an attempt has been made to distribute these animals among the other classes (Table 1, column 3). These adjusted totals are open to some objections, but they are in my opinion more meaningful than the unadjusted ones.

A primary sex ratio of 112 males to 100 females was determined from a total of 472 foetuses examined during the annual slaughters. The secondary sex ratio was not determined because the nervousness of herds

Table 1. Sex and age counts of bison recorded during the summer months, 1950 to 1953, inclusive.

Age	*Observed total*		*Adjusted total*	
	number	*per cent*	*number*	*per cent*
Calf	223	15.2	240	16
Yearling	112	7.6	135	9
Spike-horn	165	11.3	220	15
Adult and aged male	292	19.9	310	21
Adult and aged female	473	32.3	560	39
Unclassified	200	13.6	—	—
Totals:	1,465	99.9	1,465	100

with new calves precluded the prolonged study necessary to differentiate the sex of the calves. Eighty-seven spike-horns were sexed giving a ratio of 55 males to 32 females. This ratio is undoubtedly biased by the greater ease of identifying the males with their slightly larger body size and more conspicuous horns. There is no apparent reason why this ratio should depart significantly from equality. The sex ratio among adults in mixed herds (Table 1) is about five to three in favour of the females (60 males to 100 females). What part of this inequality is made up by the bull groups is difficult to state because of the tendency of the bulls to congregate along the roads and thus be counted repeatedly. It is my impression that there are too few bachelor bulls to make up entirely the deficit of bulls in the mixed herds.

Figs. 4 and 5 show in graphic form the information on herd size. Fig. 4 refers to the forested areas where the data were gathered by means of road counts in the summers only. In this habitat the herds tended to be small in size. More than one-half of the herds observed contained between six and twenty individuals and less than one-third contained more than twenty. Fig. 5 shows the frequency distribution of herds of various sizes on the open sedge meadows north of Lake Claire as counted from a plane. In this habitat the herds tended to be larger in size — nearly two-thirds of the herds contained more than twenty individuals. It will be recalled that the same tendency was noted in bull groups. Fig. 5 also shows that there is much variation in the size of the herds on the meadows from time to time, indicating that joining and splitting of herds is common. I believe that joining and splitting also occur frequently in the forests but my data are inadequate to demonstrate it there.

In both Fig. 4 and Fig. 5 the 11-20 class appears to constitute a well-marked "mode". However, since the class intervals are not all the same, a strict definition of the term "mode" does not apply. If a 1-10 class had been used it would contain the greatest frequency of observations in Fig. 4,

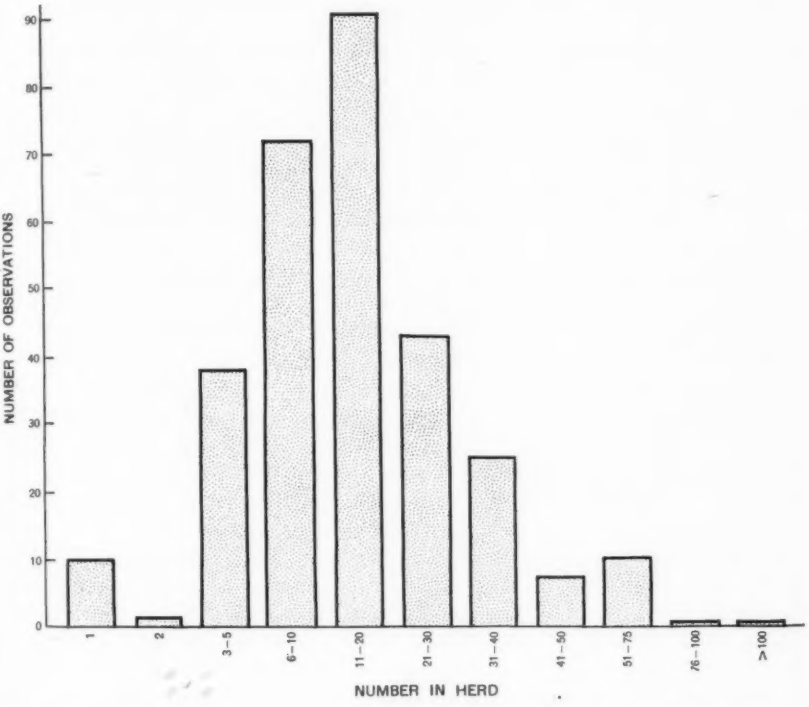

Fig. 4. Frequency of occurrence of mixed herds of various sizes in forested habitats.

but not in Fig. 5. It would also have the disadvantage of masking the scarcity of very small mixed herds (3-5), lone cows, and pairs (two cows or cow and calf).

Subject to this limitation, the data suggest the existence of a basic unit of about 11-20 individuals (possibly somewhat smaller in the forested habitat) with larger herds being formed by the transient amalgamation of two to many such basic units. This idea finds considerable support in the literature. Some authors, notably Seton (1929) went farther and believed, on the basis of purely circumstantial evidence, that the members of each unit, or clan, were blood relatives. Soper (1941) stated, without presenting any evidence, that "the various small herds and groups [of northern bison] are undoubtedly close blood relations". Garretson (1938) recognized that the large herds were formed by the close association of smaller groups, but did not believe that the smaller groups were either very stable, or blood relatives. McHugh (1958) was forced to conclude "that subgroup or group formation was flexible and depended little on blood relations beyond the age of 1 year". My data shed no light on the problem beyond pointing to the possible existence of a basic unit of some sort.

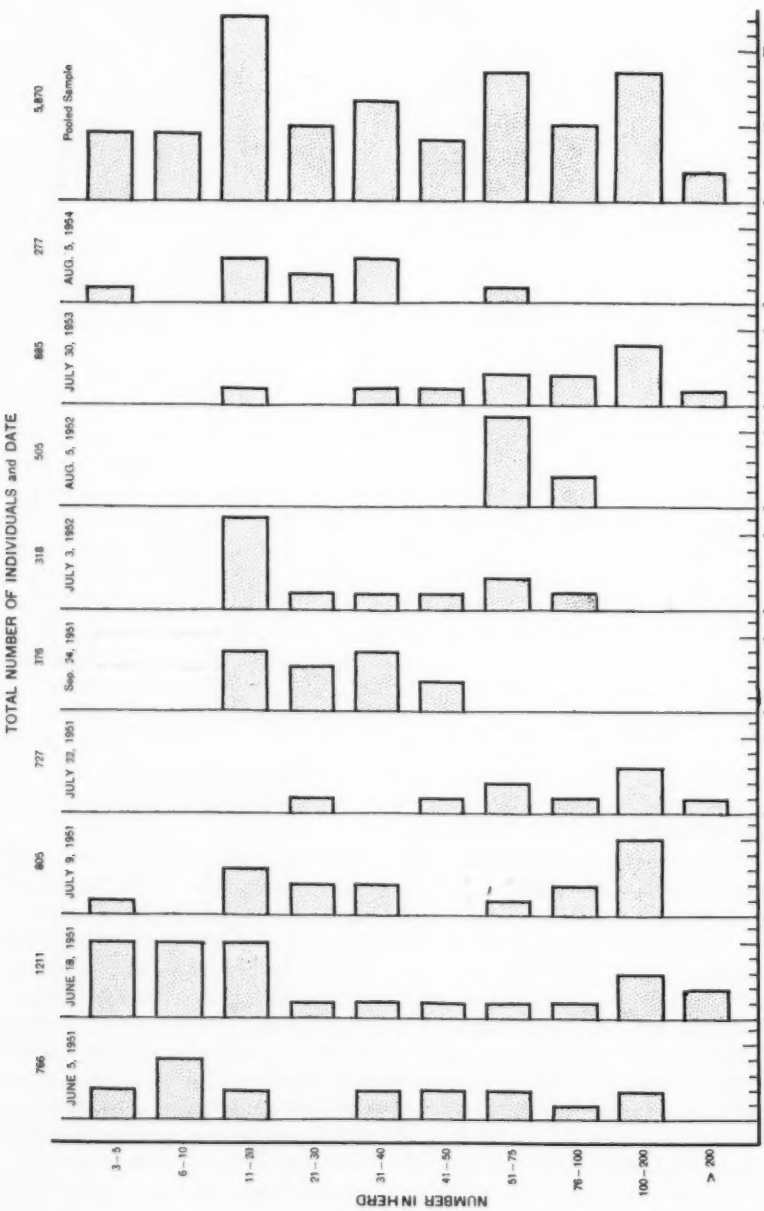

Fig. 5. Frequency of occurrence of mixed herds of various sizes on the Lake Claire meadows as observed from the air.

Intermingling of herds

In the discussion of herd size, it has been shown that there was much variation from time to time and from place to place. It is obvious, therefore, that there must be joining and splitting of groups to make larger or smaller herds. At least sometimes the union of two groups is accompanied by what amounts almost to a ceremony. This was observed three times in the field. The first observation was the most complete and will be described essentially as it was set down in my field notes for September 1, 1950.

I was stationed in the willow margin of a long narrow prairie watching a herd of 14 bison about 50 yards to my right. After about half an hour I noticed that the attention of this herd was being focussed more and more up the prairie to my left, and a few minutes later I heard bison vocalizing in that direction. Still later a herd of 18 bison emerged in single file from the timber bordering the prairie and began moving slowly toward the herd on my right. When the two herds were about 100 yards apart one bull advanced from each and the two met in a dry wallow directly in front of my position. They came toegther slowly, locked horns and pushed. This was a short encounter, without sharp impact and did not seem to be at all vicious. It ended as if by mutual agreement without any indication that one animal was victorious. The bull from the right herd then walked past his opposite number and joined the left herd, which had by this time advanced almost to the scene of the encounter. The bull from the left herd now led his group, which contained the bull from the right herd, to the end of the prairie where there was complete mingling of the animals from the two herds, without strife, but with much grunting. The bulls seemed uneasy, and walked stiffly with tails partly erect. In the next 30 minutes there was one short vicious battle between two bulls and a brief encounter between two cows. It was impossible to know the herd of origin of any of the combatants.

Two of the main elements of this incident seemed to be the preliminary meeting of two herd bulls, and the acceptance of an alien bull by one of the herds. These two elements were observed on each of the two subsequent occasions when two herds were seen to unite in this manner. The purpose seems to be recognition and the result is probably a minimum of strife when the two herds eventually coalesce.

No similar pattern was described by McHugh (1958) for any of the herds he studied, although he found that splitting and joining of groups occurred frequently. Perhaps his animals, which were confined to smaller ranges, had more frequent contact and thus recognized each other more readily.

FREQUENCY OF OCCURRENCE

Fig. 5. Frequency of occurrence of mixed herds of various sizes on the Lake Claire meadows as observed from the air.

Interspecific relations

Bison and wolves. The wolf is the only known predator of bison in Wood Buffalo National Park. In this section some observations of the behaviour of bison in the presence of wolves will be presented.

Bison appear to accept the presence of wolves without panic. I have noted this when watching herds from the air. On two occasions, our low-flying plane drove wolves from the midst of a large herd of bison. The wolves were not seen until they began to leave the herd. Nothing in the appearance or behaviour of the bison as we approached betrayed the presence of the wolves.

I have also witnessed wolf attacks from the air. On August 31, 1951, I observed seven wolves harassing a herd of nine bison, two of which were calves, on a large prairie along Murdock Creek. The bison were bunched closely together, so I was unable to tell the age and sex of the larger members. Several low passes with the plane drove away the wolves, but before we were out of sight they had resumed the attack. On my return the following day I was unable to find any sign that a kill had been made. Members of the park staff have reported similar incidents. The normal reaction of a herd caught thus in the open is to make a stand, closely bunched.

Individual bison caught in the open probably have little chance of beating off a wolf attack. The reaction of the bison in such an event is to attempt to reach the cover of heavy timber. This was noted in connection with each of four kills found during a wolf-control experiment carried out in the early winter of 1951-52. An unsuccessful attack was also recorded at this time (Fuller and Novakowski 1955). The bison probably seeks security in the forest because there he is able to dislodge any of his attackers that have secured a hold by rubbing or crushing them against the trees.

The wolves appear to single out one victim in a herd. This victim may be the subject of repeated attacks if the first attack is unsuccessful (Fuller 1957). Other members of the herd, with the exception of the mothers of calves under attack, show no regard for the presence of the wolves. The following incident, which took place on August 22, 1951, about 9 miles north of Pine Lake, illustrates this. On this occasion I stopped my truck when four bison, two bulls and two cows, appeared on the road ahead. They approached until the nearest, a cow,, was standing on the right shoulder of the road only about 100 feet away (the distances were paced off later), and a bull was slightly behind her and on the left shoulder. The other pair stopped on opposite sides of the road about 100 feet behind the closer pair. Shortly after I stopped, a wolf appeared; and in the next 52 minutes a possible fourteen wolves (minimum of ten different animals) appeared in ones and twos either on the road or in the timber alongside the road. Each time a wolf appeared, the near cow, which I soon discovered had been previously wounded by wolves, became alert. Furthermore, with each slight breeze that I could see passing through the trees, she raised her head, or looked around. The other three bison were meanwhile lying down ruminating and took no notice of the wolves. The nearer of the two bulls never ceased ruminating during the entire incident as far as I could tell. At one point, 30 minutes after the first wolf was seen, a wolf peered

over the spoilbank bordering the road not more than 25 feet from the bull and cow farthest from me. Neither of these animals looked up. The wounded cow turned apprehensively to face that wolf. The bull nearest me (i.e., farthest from the wolf, also rose and turned, but showed no nervousness and in fact continued to ruminate. Probably the presence of my truck prevented the wolves from attacking. After they departed I shot the cow and found that she had previously been wounded by a bullet and had several tears on the thighs from an earlier wolf attack.

McHugh (1958) found that bison paid no attention to a female grizzly and three cubs that captured an elk calf within 700 feet of them. Goodwin (1939) and Soper (1941) noted indifference to wolves close to the herd. However, I know of no previous accounts of the behaviour of bison that were actually being stalked by wolves. Banfield (1954) noted a parallel situation among barren ground caribou *(Rangifer arcticus)* under attack by wolves. Far from exhibiting panic, some of the animals remained bedded down when the wolves passed as close as 50 yards away.

Reactions to man. The bison has been branded a mean and, at least occasionally, dangerous animal from early times down to the present. Thus Pennant (1793) says "The bison is very fierce and dangerous" and his illustration of a bull would convince even the most sceptical reader that this was true. Cahalane (1947) says "Normally the buffalo is a timid animal, but it is unpredictable . . . rarely the buffalo is bold and aggressive, but then it can be as dangerous as a rhinoceros". With this Soper (1941) is in essential agreement. He states, after describing an example of aggression towards his dog team: "Between the extremes exemplified in the ultra bold and the excessively timid, every conceivable shade of attitude and behaviour is displayed".

I have not found bison to be either aggressive or unpredictable at any time of the year. In mixed herds I have found them usually shy and timid. The bull groups may be "stupidly dull" as described by Soper, but I prefer to believe that their outstanding characteristic could be described as "stolid indifference" (Fig. 6). Likewise I have never been impressed by the curiosity of the northern bison. It certainly falls far short of the barren ground caribou in this respect.

There is a widespread belief among residents of Fort Smith who occasionally travel in the park that the bison are dangerous. Some people have reported that they were forced to stop their cars for an hour or more by a stubborn bull that disputed the right-of-way. Others will not venture into the bison range at all during August when the rut is at its height and the bison are supposed to be most aggressive. I have never encountered these stubborn bulls, nor have I observed increased aggressiveness toward people and cars during August. There is, at this time, however, more reluctance on the part of the bulls to give way. Even in August I was always able to dominate any bull I met although my English car weighed less than a full-grown bull.

Fig. 6. Bison bull in attitude of "stolid indifference".

During the course of this study I found myself in only two potentially dangerous situations. At 2245 on August 27, 1951, the truck that I was driving, a Dodge Power Wagon, was charged by a bull, which hit the bumper and left front fender with sufficient force to push the truck backwards up a slight incline. The animal then swerved away into the forest. I believe this animal was merely trying to escape and was blinded by the headlights of the truck that I had not had the presence of mind to extinguish. Had this not been so, the bull probably would have done additional damage to the truck. A more dangerous incident took place on August 10, 1951. D. R. Flook, Canadian Wildlife Service, and I were stalking a herd along the edge of a small meadow. We had passed some animals on the edge of the herd when we unexpectedly met a cow in the willows. When the cow wheeled and ran, a stampede for the timber was precipitated. We were in the line of retreat of six animals that had been feeding on the meadow. They came crashing through the willows directly for us but split into two groups of three and passed only 6 to 10 feet on either side of us when I shouted and waved my cap. Again, it is quite obvious that we were accidentally in the way and were not the victims of a premeditated attack.

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ON THE IMPORTANCE OF THE PHONEMIC PRINCIPLE IN THE DESIGN OF AN ORTHOGRAPHY

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Introduction

IN A previous article in this journal (*Arctic* 12:203-13) the fundamental importance of the phonemic principle in the devising of a standard spelling for the Eskimo language spoken in Canada was referred to, but without elaborating its meaning and function. The main purpose of the present paper is to show the validity of this basic principle by giving a more detailed explanation of its role in language writing. This would be unnecessary if the creation of a system of writing concerned linguists only, since they generally agree that a phonemic description not only reveals the basic functional sound structure of the language in question but also acts as a practical orthography. However, though the linguist has a leading role to play, such a vast and complex project demands the attention of groups of people of widely diverse background and training, namely, administrators, missionaries, anthropologists, teachers, linguists, and native leaders. In the initial stages the main responsibility rests on the linguist who must act as architect and draw a master plan which can serve as a framework around which the contributions of all others can be consolidated. Therefore, the first step to be taken is to make a scientific analysis of the phonological or sound structure of the language in question and to establish on this basis a spelling that is simple, accurate, and efficient; in a word, economical. The purely scientific aspect of the question offers many problems in itself; however, because of the human factors involved, the successful realization of such a plan in a socio-political situation is far more difficult. A free exchange of views from all quarters is essential, and this can best be realized if there is a common understanding of the theoretical basis of the linguist's recommendations. What then is the phonemic principle?

The phonemic principle: definition and application

The phonemic principle can obviously not be understood without first defining the term phoneme. This, in turn, can best be explained if the concept of language as a structure or a hierarchy of structures is made clear at the outset. Structuralism is a point of view that revolutionized linguistic research and gave birth to linguistics as a science pursuing goals quite distinct from traditional philological studies. This movement began

some forty years ago with Ferdinand de Saussure's brilliant idea that language is a dual reality, which he termed *langue* and *parole*. To avoid certain misleading connotations of these two terms, they will be replaced by *code* and *message*, respectively, in what is to follow. For de Saussure, what is heard in every day speech is but the realization of a system of rules which exists in the mind of the speaker independently of the substance or content used to actualize them. This is the *code*, which can function, or better, which has a potential function, irrespective of the quality of its material units. For example, the code or rules of the game of chess would remain unchanged even if we replaced a set of ivory chessmen by small pyramids of different sizes, colours and substances. The modifications of various aspects of the code of languages in contact that are taking place for instance in bilingual Greenland show how one code can influence another without any reference to substance. The morphological and syntactic structures of the Greenlandic spoken by some bilingual natives bear the mark of certain rules of Danish morphology and syntax. A case in point: one of my Greenlandic informants, who is well educated in both his native language and Danish, gave two translations of the Danish sentence *du gaar ud ikke* "you do not go out", namely, *aníngilatit* and *ivdlit anivoq nâmik*. The first translation is the characteristic Eskimo word-sentence literally meaning "go out not you" whereas the second is essentially a Danish construction literally meaning "you go out not". Apparently this particular foreign construction first manifested itself about a generation ago and seems to be gaining ground.

What are the precise Danish structural elements in the sentence *ivdlit anivoq nâmik*? First, the personal pronoun *ivdlit* "you" normally only an emphatic form as in *ivdlit aníngilatit* "you, you do not go out", is simply a literal translation of the non-emphatic Danish pronoun *du* that would ordinarily be translated by the suffix *-tit* in such word-sentences as *aníngilatit*. The influence of the morphological and syntactic structures of Danish is illustrated by the change of nature and position of the suffix *-tit* "you" transformed to *ivdlit*, an independent word in pre-verbal position like the Danish personal pronoun *du* "you". Secondly, in standard Greenlandic, the suffix *-voq* of *anivoq* "he, she, it goes out" corresponds to the three pronouns. In the sentence in question this suffix has lost its pronominal meaning and consequently, *anivoq* plays a purely verbal role equivalent to the Danish verb *gaar ud* "go out". Furthermore, *anivoq* like *gaar ud* remains unchanged in the three persons singular and plural. Finally, the negative *nâmik* found only in pre-verbal position in standard Greenlandic, here follows the verb *anivoq* like the Danish negative *ikke* follows *gaar ud*.

Much the same story can be told in the realm of phonology or the structure of the basic functional sounds of the code. The Eskimo phoneme /j/ occurs only intervocalically but under the pressure of English loanwords of high frequency such as *Jesus, Johnny, Jacobie*, etc., this rule of phonemic distribution is changing for the native pronounces /j/ (equivalent

phonetically to the *y* of English *yes)* at the beginning of such new words in his vocabulary.

These systems of rules or linguistic structures, namely, the phonemic structure (the nature and behaviour of phonemes or basic functional sounds), the morphological structure (the distribution of affixes, for example, such as *-ness* in English which can only occur at the end of words but in some words can be followed by a plural suffix) and the syntactic structure (the order of words in a sentence) constitute the three main structures of the code of a given language. The phonemic structure will be the only one of these dwelt on in the main body of this paper. As opposed to these structures or code is the *message* which is the MEANINGFUL expression of these structures in the concrete form of speech. The code, therefore, with its hierarchy of structures is a well-ordered system of rules with potential functions that have practical use only when a given set of phonemes known to more than one person are utilized for the purpose of communicating meaning. The code — the invisible reality of linguistic structures existing independently of substance and meaning — is acquired by all of us in the long process of learning our mother tongue by dint of constant repetition. Even the most unlettered are in possession of this hidden reality, for we all learn to speak before we learn to write. Illiterate people are still in the majority in the world today and even though they may not be able to give the enquirer a complete picture of the code they use, simply because they never felt the need to make a detailed analysis of it, they nevertheless use it effectively in every-day speech contrary to the opinion of many who think that a language cannot function adequately without a heavy dosage of formal bookish grammatical training. The code, which is passed on from generation to generation through the medium of the message (the meaningful content), is the invisible property of everybody before it becomes, in book form, the visible property of grammarians, school teachers, and other linguistic norm-makers.

For evidence that the various structures of the code exist independently of content one only needs to turn to Lewis Carroll's *Jabberwocky* with its profusion of nonsense words. Here are the first few lines:

'Twas brillig, and the slithy toves
Did gyre and gimble in the wabe;
All mimsy were the borogoves,
And the mome raths outgrabe.

This is English and not Greek, although because of the nonsense words the meaning is rather obscure, if not completely lacking. Of course, had Carroll so chosen, he could have excluded all intelligible English words from this poem without affecting the code of the English language one iota. The immense popularity of this nonsense verse among English-speaking people proves its "Englishness", which consists of its following to the letter the rules of English phonemic, morphological, and syntactic structures. Had Carroll written, for example, *rbillig* and *vtose*, instead of *brillig* and *toves*, his English readers would have instinctively rebelled against such

phonological monstrosities for the simple reason that *rb* is inadmissible at the beginning of an English syllable and *vt* is impossible in all positions. The same applies on the levels of morphology and syntax. Had Carroll left out the *-s* suffix of *borogoves* most English speakers would feel slightly uncomfortable in reading and quoting *all mimsy were the borogove,* largely because in 99 per cent of cases a plural morpheme is expected at the end of the subject of the verb *were,* as in *so hot were the stoves* . . . It is true that the existence of *sheep, deer,* and *moose* and the other irregular plurals as *man/men* in some very small measure would lessen the doubt of the speaker in using the singular subject with *were* but this factor is negligible owing to the low frequency of these irregular forms. And finally, if the author of these lines had put the verb *did* immediately after *gyre* the nonsense value of the verse would have increased. In the same way, if the first *and* had come immediately after *toves* a similar non-English syntactic effect would have been obtained.

To summarize, it can be seen that the structuralist views language as a dual reality — the code and message — where order, system, and structure predominate, that is, where every piece fits into an ordered whole that in turn fits into a larger whole; in a word, where everything hangs together in place. In this connection it is pertinent to state the point of view of André Martinet, a leading structuralist: "The . . . fundamental aspect of the phonological discipline and those related to it, is the concept of language as a structure, or better as a structure of structures, in the sense that each of the linguistic elements is not conceived as autonomous, but as interdependent with other elements belonging to the same functional type, in such a way that language must not be viewed as a simple conglomerate of independent units whose nature and behaviour might be studied independently of that of their neighbours." (Lingua 1, p. 39. Translation mine.).

If the chief aim of language is to communicate meaning between individuals through the medium of vocal sounds, similarly the chief aim of a system of writing is to communicate meaning through the medium of visible symbols. Of course, the written text cannot be an exact copy of the infinite variety of sounds produced in speech, that is, in the message. On the contrary, it should as much as possible be a replica of a definite and limited number of basic functional sounds, that is, of the phonemic system of the language as it exists in a more or less ideal state in the mind or nervous system of the speaker. Linguists view the phoneme as an abstract entity existing on the level of the code, having the possibility of multiple realization in speech according to its immediate phonetic environment, the mood of the speaker, and the physiological make-up of his vocal organs. Laboratory experiments have demonstrated that the number of potential sounds in a language approaches infinity. With the application of the phonemic principle this complexity of non-functional sounds can be reduced to a small number of functional signals or phonemes (25 on the average) that make up the units of the phonemic structure of the code.

Not only is the speech of a given individual varied, uneven, and irregular but also, as sensitive measuring instruments show, every sound in his utterances is unique, even when he is repeating the same words. Fortunately, this complete chaos is more apparent than real, for it contains an ordered system of basic functional sounds or phonemes whose function is to distinguish meanings between words. For example, when *p* and *b* are opposed in similar phonetic environments in *pill* and *bill* it is evident that the meaning of these two English nouns is distinguished only by their initial sounds. Since these have a function they are called phonemes. Each phoneme has a fairly wide range of realization, the limits of which must not be exceeded without danger of ambiguity. A *p* which receives too much voice ceases to be a voiceless stop and becomes a *b*, that is, a voiced stop. Of course, if the voiced/voiceless opposition has no function in the sound system of a given language it means that the bilabial stop will double its range of realization. It will be heard sometimes as a voiced stop and at other times as a voiceless stop. At this point it might be well to mention that the term *phonetic* is used by structuralists to refer to speech sounds aside from their function. For example, the aspirated sound following the *p* of *pill* plays no role in distinguishing meanings in English. The term *phonemic* always refers to functional sounds such as the *p* and *b* in *pill* and *bill*. It is very important to note that these two bilabial stops — *p* and *b* — are not necessarily phonemes in other languages. In some Amerindian languages these two English phonemes are heard but do not have the function of distinguishing meanings between utterances. They occur in free variation, since their only distinctive feature — their voiceless/voiced opposition — is not relevant to keep meanings apart; in such a language it would not matter whether *pill* were pronounced *bill* or vice versa, since they would be merely variants of the one word with the same meaning. In certain languages *p* and *b* occur in complementary distribution or according to their phonetic environment. For instance, *p* would occur only before other voiceless sounds in such words as *ipfi, ipsa,* etc., and *b* only before voiced sounds in such expressions as *ibvi, ibza,* etc. In such a language *p* and *b* would not be phonemes, that is, separate sounds with distinct functions, but rather, they would be different members of the same family of sound or phoneme, which might be written either as *p* or *b* or any other symbol for that matter. These two sounds that vary according to their phonetic environment would be called allophones, because they do not oppose each other in the same phonetic context in the way they do in *pill* and *bill* in English. What would keep the meanings of ipfi/ibvi apart in such a language would be the /f/ and /v/ phonemes, which would have been observed to occur in functional opposition in other contexts such as in *afo/avo,* whereas *p* and *b* would never be found to oppose each other in similar environments in such non-existing pairs as *ipfi/ibfi* or *tapso/tabso,* for example.

The difference between phonemes and allophones can perhaps be more readily seen in examples taken from English. When an Englishman utters

spot and *pot* he is not at all aware that the *p* in each word is different. The *p* in both is a voiceless bilabial stop but the *p* of *spot* is unaspirated whereas the *p* of *pot* is aspirated, that is, it is accompanied by a slight puff of breath somewhat like the *h* sound of English in *help*. The difference in pronunciation of these two stops is not noticed by the native speaker because in the English phonemic system the distinctive feature of aspiration/non-aspiration does not exist to distinguish meanings as it does in the Cree Indian language, for example. Native speakers are not conscious of allophones but usually are of phonemes. It is the foreigner who is capable of spotting easily the allophones of another language when these happen to coincide in phonetic content with the functional sounds of his language. This happens automatically, much to his misfortune, for therein lies the root of the difficulties in learning a foreign language and reducing to writing a language which is not one's mother tongue.

To clarify this point further, it might be well to bring back to the reader's attention that in my last article in this journal I discussed at greater length the phenomena of over- and under-differentiation of phonemes as two of the major obstacles people face when languages are in contact. Each of us tends to interpret the phonemic and other structures of a foreign language according to his own. The Cree Indian learning English would automatically assume that the *p* of *spot* is different from the one in *pot*, in effect, that they are two different phonemes, simply because there is an aspirated *p* phoneme in Cree that stands distinct from an unaspirated *p* phoneme, thus distinguishing such pairs as *pukan* "a nut" from *puk'an* "separate." This is over-differentiation of phonemes, that is, attributing to a language more phonemes than it has in reality. On the other hand, an Eskimo learning English would tend to interpret such pairs of phonemes as *p*/*b* and *t*/*d* as being one and the same sound or phoneme respectively, because in his phonemic system only *p* and *t* exist as functional sounds and not their voiced counterparts. As a matter of fact, the voiced sounds *b* and *d* of English would escape his notice and he would pronounce *bill* as *pill* and *do* as *to* until such a time when the resulting ambiguities would oblige him to make an effort to the contrary. This is under-differentiation of phonemes, that is, attributing to a language fewer phonemes than it has in reality. Since writing, ideally speaking, should reflect exactly the phonemic structure of the language in question, the system of writing is bound to suffer when a language is reduced to writing by someone who has not fully or accurately grasped its phonemic reality through a lack of objectivity. The existing Eskimo orthographies all suffer in this way because those who designed them superimposed on the Eskimo phonemic structure certain features of their own phonemic system through the normal, automatic, and unconscious process of the over- and under-differentiation of phonemes when different linguistic structures come into contact. As a result, the phonemic reality of Eskimo was vitiated or, as one critic calls it, mutilated and disfigured. The ideal application of the phonemic principle in the design of a spelling system is that it should never have more symbols than the

number of phonemes found in the language being reduced to writing, that is, that there should be a one-to-one correspondence between the phonemes and their symbols. For example, the use of the five vowels — *a, e, i, o, u* — in almost all alphabetic spellings of the Eastern Eskimo dialect group, which has only three vocalic phonemes, warps the reality of the Eskimo phonemic system that alone should be portrayed in the writing.

Such words as *vitiated, mutilated, disfigured,* and *warped* used to describe the inadequate portrayal of the phonemic reality of Eskimo by the present spelling systems may seem strong but the point is crucial. Although certain exceptions to the phonemic principle of a practical nature may exist in a given language, it is nevertheless *de rigueur* to try to achieve in a system of writing the exact image of the phonemic structure of a given language, since they are but the two facets of the same entity or body of meaning that communicates itself by coming to life through the unruly speech continuum. The speech stream is made imperfect by the physical exigencies of the articulatory apparatus that must produce a series of diverse phonemes pressing hard one upon another, by the modifications of each phoneme by its immediate phonetic environment, by the physical condition of the speaker's vocal organs, and finally, even by his mood. The identity of the two facets just mentioned — the phonemic structure and the orthography — might be more easily visualized by comparing the free spontaneous speech utterance of a given individual with his reading the same utterance from a text written in a perfectly phonemic alphabet. In this case the spelling would be the exact image of the phonemic system of his language. Upon making a particular utterance such a person would be translating only imperfectly into audible sounds the inner, invisible, and efficient system of abstract signals (phonemes) that forms one of the structures of the code of his language. Upon reading the same passage written in an economical and efficient alphabet he would be performing exactly the same action as described above for ordinary speech, with one exception that in no way alters the identity of the two facets of the same reality. In spontaneous speech the utterance is the result of an inner and invisible process of arrangement of linguistic units, whereas in reading the arrangement is an outer and visible one that has already been materialized in print and is merely waiting for someone to breathe life into it. In other words, speech, whether prompted by the printed word or the inner processes of thought, is the same imperfect medium of realization of the more or less perfect code. More light can be thrown on this question in the words of Daniel Jones, the renowned British phonetician: "Viewed 'psychologically' a phoneme is a speech sound pictured in one's mind and 'aimed at' in the process of talking. The actual concrete sound (phone) employed in any particular speech-utterance may be pictured sound or it may be another sound having some affinity to it, its use being conditioned by some feature or features of the phonetic context. This was the view taken by BAUDOUIN DE COURTENAY and his immediate followers. BENNI told me (about 1913) that they consequently recognized two kinds of

phonetics: one was called by them 'psychophonetics' and related to the pictured sounds; the other was called 'physiophonetics' and related to the concrete sounds actually uttered. Corresponding to these were two types of phonetic transcription: the 'psychophonic' (representing only phonemes) and the 'physiophonic' (representing sounds actually uttered)." (Suppl. to "Le maître phonétique", Int. Phon. Assoc. 1957, p. 7).

Broadly speaking the existing alphabetic spellings of Eskimo represent a "physiophonic transcription" or the sounds actually uttered or thought to be uttered according to the phonemic system of the mother tongue of the designer of the orthography. Everyone who has ever attempted to speak a foreign language knows the difficulty of articulating new phonemes, that is, sounds that do not exist in his mother tongue. The problems that the two *th* phonemes of English as in *thin* and *this* create for non-natives learning English who do not have these phonemes in their language is proverbial. When a French-Canadian pronounces *thank you* as *tank you,* it is because he reproduces the phoneme of his mother tongue which is nearest in sound and point of articulation to the new phoneme; the French /t/ is a voiceless dental stop whereas the English /th/ phoneme as in *thin* is a voiceless interdental fricative and both are articulated in roughly the same area. In the same way, the French-Canadian usually says *dis* for *this,* the same process of mistaken identity taking place.

The /g/ and /r/ phonemes of Eskimo create much the same problem. An English-speaking person most often interprets the Eskimo /g/ (a fricative sound non-existent in English) as the English velar stop /g/ as in *go.* Both are articulated in the same area of the mouth in Eskimo and English, the only difference being that where the back of the tongue touches the soft palate or velum in the English articulation, the tongue merely comes close to the soft palate in the case of Eskimo, leaving a small passage for the friction of air to pass through. It is interesting to note that the French guttural *r* heard commonly in Quebec City and Paris is very close to the Eskimo uvular /r/ phoneme, and consequently, Frenchmen from these regions have no trouble identifying and reproducing the Eskimo /r/. But this is not so with the English person, whose language has neither the Eskimo /r/ nor the French guttural *r.* As a result, the English usually interpret the Eskimo /r/ as their /g/ as in *go* because here again, this is the English phoneme nearest to the point of articulation of the Eskimo /r/. In other words, two separate fricative Eskimo phonemes /g/ and /r/ are bundled together as one English stop /g/ in English ears. For written evidence of this under-differentiation of phonemes, one can refer to the *Eskimo Bulletin* published by the Department of Northern Affairs and National Resources in Ottawa. It will be noticed that in this publication the /k/ and /q/ phonemes of Eskimo also are not distinguished, both being written with the letter *k.*

Perhaps the best way of emphasizing and clarifying the ever-present dangers of deforming the phonemic reality of a foreign language through the superimposition upon it of one's own phonemic structure might be

by reversing the roles. Let us suppose that English were an unwritten language and the task of reducing it to writing were left to the Eskimo people. If the Eskimos in charge of this project were laymen who had no knowledge of linguistic theory, the following are some of the errors they would be likely to make. As mentioned earlier, they would confuse such pairs of phonemes as English *p/b* and *t/d* and symbolize each pair with one letter only, *p* and *t* or *b* and *d*, respectively, probably depending on which set of letters they used to write their bilabial and dental stop phonemes in Eskimo. At first the nine vocalic phonemes of English in contrast with the three Eskimo vowels would create some confusion. It would be easy enough for them to tell the difference between *beet* and *bit* because they too distinguish between long and short or tense and lax vowels. However, the difference between *bit* and *bet* would create more of a problem. These two vocalic sounds exist as allophones or members of the same family of sound — the phoneme /i/ — in Eskimo, and thus do not need to be distinguished in the spelling because they never mark differences of meanings between utterances. The likelihood is that the Eskimo laymen in question would decide not to distinguish the vowels of *bit* and *bet* in the spelling, because they would hear them as one and the same sound. Of course, if the functional yield of these two phonemes were so high that it created many cases of homonymy not clarified by the context, then the two phonemes would have to be symbolized by two different letters, otherwise there would be too much confusion in reading an English text so devised. If in their under-differentiation of English phonemes the Eskimos chose to write the following phonemic pairs — *p/b*, *t/d*, and *i/e* — as *p*, *t* and *i* respectively, it would mean that what we now write as *bed* would be spelled *pit* so that the sentence *I like sleeping in a pit*, for example, would prove strangely ambiguous in the new spelling.

The important thing to remember is that the errors and inaccuracies of the Eskimo spelling systems that are due to the under-differentiation of phonemes, though flagrant enough, did not create insurmountable problems in comprehension (for the natives at least), because of the enormous assistance given to them by the low functional yield of the under-differentiated phonemes in question and the help of the context. In the Eskimo-devised spelling of English just referred to, such an utterance as *I pit Puster pit that pig pad poy*, as odd as it seems to English eyes, could probably be read to mean *I bet Buster bit the big bad boy* by most English-speaking persons, even with so limited a context. For instance, *pit* in the sense of *bed* could not logically fit into the above sentence. Furthermore, it must be noted that in the writing systems of Eskimo that suffer from over-differentiation of phonemes (five vowels instead of three, for example), this over-abundance of symbols did not hinder comprehension, but it severely burdened the written language with unnecessary letters. Everybody knows the difficulties of English and French spelling in this connection, especially for those learning to write. The same problem applies for the Eskimos and they often ignore in their writing the extra letters

symbolizing allophones. For instance, I was shown a letter written by a young Eskimo girl to Father L. Schneider, O.M.I., who had taught her his own five-vowel system. She had sometimes ignored the unnecessary distinctions between the allophones of the Eskimo phonemes /i/ and /u/ that are symbolized by the four letters *i* and *e*, *u* and *o*, respectively in the alphabet she had learned. Of course, she had been taught certain rules for their correct use but since these were superfluous, in that they served no function, it was much harder for her to remember them. Edward Sapir, the eminent linguist, who had a wide experience with the Amerindian languages and peoples, claimed that people possess "phonemic intuitions" which reveal themselves as soon as they begin to write their own language alphabetically or phonemically and that they instinctively ignore the allophones (the variants of one phoneme) in their writing unless taught otherwise, and even then as we have just seen, there is some resistance. These "phonemic intuitions" which come into play when the native is confronted with the task of writing are the concrete manifestations of the existence of that hidden reality — the code — or part of it, the phonemic structure which is firmly rooted in the mind of the speaker from years of constant repetition and practice. Therefore, to the Eskimo who says /*imi*(q)/ "fresh water" there are really only four sounds (or three if the final consonant is not pronounced as in some dialects) which have a function. When he wants to say "fresh water" he must be sure to select /i/ as the first vocalic phoneme in order to avoid confusion with /ami(q)/ "skin". In the same way, he must choose /m/ in preference to /p/ or /s/ to keep "fresh water" distinct from /ipi(q)/ "dirt" and /isi(q)/ "smoke". Finally, the second vowel /i/ is chosen to distinguish /imi(q)/ from /ima(q)/ "sea, salt water". If the final consonant /q/ has almost disappeared in the pronunciation of certain dialects it is largely because this sound no longer serves a function to distinguish meanings in this position. Therefore, in this so-called automatic selection of sounds, which a native speaker makes, it is a choice of *phonemes* and not of allophones that is made; the native speaker is never conscious of the latter but only of the former, which may be modified in the stream of speech for the reasons mentioned, namely, the exigencies of articulation and the immediate phonetic environment of the phonemes. It might be well to turn to English once again for examples to clarify this notion further.

Daniel Jones defines the phoneme as: "a family of sounds in a given language which are related in character and are used in such a way that no member ever occurs in a word in the same phonetic context as any other member" or to use Swadesh's terse phrase, where all the members of each family of sounds are in "complementary distribution". Up to now, two levels of language have been spoken of, the ideal structure that exists in the mind or neuro-muscular system of the speaker and the meaningful actualization of that structure in the speech continuum; in other words, the code and the message, respectively. Although the above definition of the phoneme by Jones, inasmuch as it refers to a family of sounds whose

members are realized on the physiophonic level or in speech, might lead us to believe that the phoneme is nothing but the sum total of the members (allophones) of the family of sound, the phoneme nonetheless exists as an abstract entity on the psychophonic level. In a very real sense, the phonemes dwell as abstract and integral sound units on the psychophonic level and never reveal themselves as such on the physiophonic level. When actualized in speech they manifest only different facets of their multiple personality depending largely on their phonetic environment.

When Jones speaks of a family of sounds, the various members of this family are the various facets, found in speech, of the integral psychophonic entity "aimed at" unconsciously by the speaker. For example, when we utter the three English words *kill, cool* and *call,* the final *l* sound in each case is different. Experimental phonetics has shown this to be true with the use of sensitive instruments that record the sounds graphically. The native speaker is not aware of these differences because his sole intention is to reproduce vocally the inner reality of the phoneme /l/ which exists in his phonemic system, his intention being forcibly modified by the neighboring sounds. The *l* of *kill* follows a high front vowel and therefore differs from the so-called dark *l* of *cool* which follows a high back vowel (a velar vowel) that in drawing the *l* back gives it a velar or dark quality. In turn, the *l* of *call* preceded by a low back vowel differs from the other two owing to a larger area of resonance brought about by the lowest possible position of the tongue in the production of the vowel *a*. These three varieties of *l* are not phonemes in English because they do not serve to distinguish meanings between words. They are members of the *l*-family of sounds which can be shown to be a phoneme by opposing such pairs as *cool* and *coop, kill* and *kid, call* and *caught.* In each of these the *l* distinguishes meanings in similar phonetic environments. Therefore, on the psychophonic level *l* exists as a phoneme of English that on the physiophonic level realizes itself in three ways depending on the phonetic quality and point of articulation of the vowel preceding it. These different realizations are allophones or members of the same family of sounds — the phoneme /l/ — the integral entity and ideal sound aimed at in the process of speech.

Conclusion

If we accept the distinction between *code* and *message* (speech) or between *psychophonic level* and *physiophonic level* (speech), and that the speech continuum of a given language is only an imperfect realization of the well-ordered inner reality — the code — and especially of the phonemic structure containing all the necessary elements that permute and combine in opposition to each other to express meaning, it would seem logical that an orthography whose main purpose is to symbolize the same meaning in visible form, should as much as possible be a reflection of the phonemic structure of the code. In other words, if meaning is created by the various permutations and combinations of basic functional sounds or phonemes, these alone and only these need to be symbolized to capture the same

meaning in visible form in a writing system.

It should be pointed out that there are different schools of thought on the subject of phonemic descriptions, but that in spite of this, experience has shown that the actual results of these different applications of the phonemic principle are surprisingly similar.

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ARCTIC ICE ISLAND AND ICE SHELF STUDIES

Part II

A. P. Crary

In Part I of this article (Crary 1958) the thicknesses, surface and subsurface features of the Ellesmere ice shelf* near Ward Hunt Island and the ice island T-3 were outlined. The purpose of Part II is to discuss the possible conditions at high latitudes (north of about 80°N.) under which thick floating ice can form, and to outline the possible history of these two thick remnants of arctic ice. A further purpose is to explain their general surface features and to discuss briefly the possible future of these ice masses.

T-3 consists, away from the edges, of two general types of ice: the upper part with innumerable dirt layers and the lower part without these dirt layers. Marshall (1955) and Weeks (*in* Crary 1958) have both examined petrographic evidence from the upper part of the lower ice and have reached the conclusion that this is not sea ice. However, the lack of any traces of dirt in the lower ice (when all ice that has obviously accumulated from the top shows many dirt layers) and its salinity indicate natural growth from below by freezing, perhaps of brackish or fresh run-off waters of coastal streams. The presence of well-preserved shells found above sea-level along the northern shores of Ellesmere Island, which pre-date pieces of driftwood located back of the present shelf and therefore pre-date the shelf, make it appear most improbable that glacier ice forms the base of the main shelf feature. Local glacier ice does, however, play a part in the formation of some ice shelves and this has been discussed by Marshall (1955).

Growth of floating ice sheets in Arctic Ocean areas

The growth of perennial ice can take place either at its top surface by the accumulation of snow or at its lower boundary by the freezing of sea water. The upward flow of heat from the lower boundary is caused by the average difference in temperature between the ice surface and the water and removes the latent heat released by the freezing of the sea water. The amount of growth is limited by the amount of surface ablation and by any additional heat that may be supplied by the ocean waters.

* Since this article was written the Canadian Board on Geographical Names has adopted several new names and name changes for northern Ellesmere Island. They have been used in the illustrations, but not in the text.

For our considerations only net annual changes in high arctic areas will be considered. It will also be assumed that the heat flow through the ice can by calculated using the average annual surface temperature. The annual heat flow per cm.^{2}, Q, through an ice sheet H cm. thick is given by

$$Q = \frac{K \triangle T}{H} t \qquad (1)$$

where K is the thermal conductivity, $\triangle T$ the difference between the average annual surface temperature and water temperature, and t the time, 1 year or 3.1×10^7 sec.

The thermal conductivity of fresh-water ice is 0.0053 (in C.G.S. units). Malmgren (1927), from observations made on sea ice from the *Maud*, reported values varying from 0.0015 near the surface to 0.0045 near the bottom.

The indirect Schmidt approximation method *(see* Ingersoll *et al.* 1954, p. 209-11) has been used to check these thermal constants for shelf and floe ice from known changes in ice temperatures. By using a time interval of 1 day and an arbitrary depth interval varying from 30 cm. to 70 cm. the value of diffusivity dependent on these time and depth intervals was made to vary as widely as necessary. Fixed upper and lower levels were chosen where temperatures were measured, and these were varied in the calculations according to the observed data. Interpolations from measured temperature-depth values were used for the initial boundary conditions, and after calculations the theoretical and observed temperature-depth curves were compared. Fig. 1 shows these comparisons for the two cases computed: (1) fresh ice in the upper part of the Ellesmere ice shelf, (2) a 2.5-metre sea-ice floe that was studied by Iakovlev (Somov 1955) during the drift of Station NP 2. In the ice shelf study the best fit is obtained using a diffusivity of 0.0121 (in C.G.S. units), which gives, using 0.9 for the density and 0.5 for the specific heat, a value of 0.0054 for the thermal conductivity. In the ice floe study a good fit is obtained with a diffusivity value of 0.0096. Using again 0.9 for the density and 0.6 for the specific heat for ice of this salinity and temperature, the thermal conductivity would be 0.0052 (in C.G.S. units). A value of 0.0053 is used here for the calculations of ice shelf growth.

The value of $\triangle T$ will be taken as 15.2°C., the difference between -17°C., the average annual surface temperature and -1.8°C., the freezing point of the surface water of the Arctic Ocean. The general solution can be written as follows:

$$\frac{dH}{dt} = S + \left[\frac{K \triangle T}{H} - A \right] \frac{1}{\rho L} \qquad (2)$$

where S represents the surface change, either positive as accumulation or negative as ablation, A represents the heat furnished by the ocean waters, ρ the density or 0.9, and L represents the latent heat released in freezing.

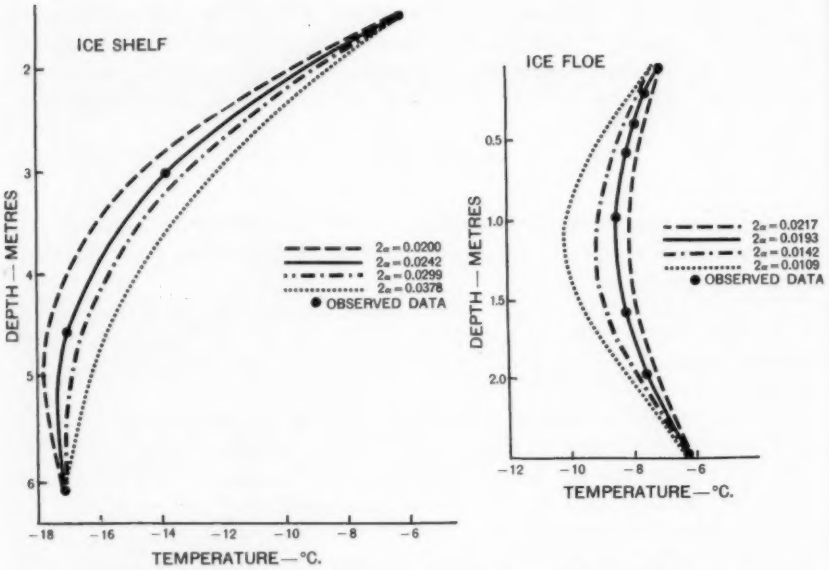

Fig. 1. Temperature-depth curves based on Schmidt approximation.

This equation can be simplified to:

$$\frac{dH}{dt} = \frac{a}{H} + C \qquad (3)$$

where $a = K \triangle T/\rho L = 0.0011$ and $C = S - A/\rho L$. The solution of this equation assuming that $H = O$ when $t = O$, is

$$t = \frac{H}{C} - \frac{a}{C^2} \ln \frac{a+CH}{a}. \qquad (4)$$

This gives the time, t, necessary to form ice of thickness H.
When $C = O$, the solution of equation (3) is

$$t = H^2/2a \qquad (5)$$

an approximate formula widely used for ice growth.

Fig. 2 shows graphically the time in years necessary to grow ice of various thicknesses with different annual values of C. It can be seen that 50 metres of ice could be built up in 360 years and 100 metres in 1440 years without net change at the surface and without heat supplied by the sea water. With an accretion of 5 centimetres per year a thickness of 50 metres would be reached in 245 years and a thickness of 100 metres in 760 years.

Equation (3) shows that no growth of ice can take place if $C \leqq -a/H$. This determines the combinations of surface conditions and heat from the ocean waters that limit the ice thickness. Fig. 3 shows the maximum or

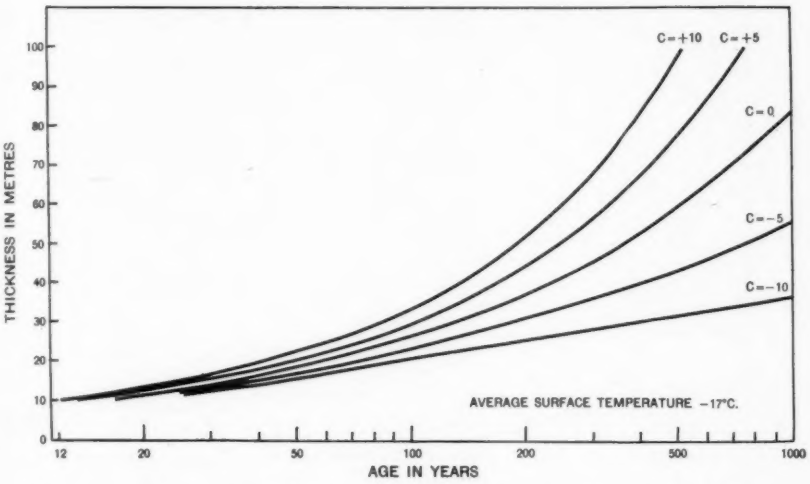

Fig. 2. Time necessary to graw ice of varying thicknesses with various values of C.

equilibrium values of ice thicknesses associated with various values of surface ablation and heat contributed by the ocean waters. The time required to establish equilibrium is sufficiently long for minor climatic changes to occur. Thus, if C is not generally constant but varies over the years consequent variations in ice thickness may be expected.

Fig. 4 gives the accumulation and ablation observed on T-3 at latitudes 83° to 89°N. and on the ice shelf at 82°N. during the years 1952 to 1955. The high arctic areas are relatively arid and the small accumulation figures at T-3 are substantiated by the data given in Table 1. When the summer melt water remains on the surface of the floating ice and by freezing adds to its growth then there is a partial lowering of the surface temperature and a corresponding decrease of the average annual temperature difference between the upper and lower ice boundaries. Although in the years 1952 to 1955 there was a net ablation of the ice surface, the ice shelves and sea-level ice fields in high arctic areas indicate that the average surface change over many centuries has not been great.

Little information is available on the role played by various factors in the thermal budget of the ice surface of the Arctic Ocean. Iakovlev *(in* Timofeev 1958) has computed the heat balance of the ice cover at the NP 2 Station over a period of a year, finding the radiative balance of the surface to be + 2.9 kcal./cm.^{2}, turbulent heat exchange between ice and air + 1.5, heat of melting — 2.9, heat flux from water + 5.5, and evaporation — 7.0, this last figure being obtained as a residual value. At the IGY drifting station Alpha a little farther north Untersteiner (1958) found by direct measurement of the summer thermal budget that the maximum evaporation loss over a period of 12 hours was only 30 cal./cm.^{2}. Untersteiner

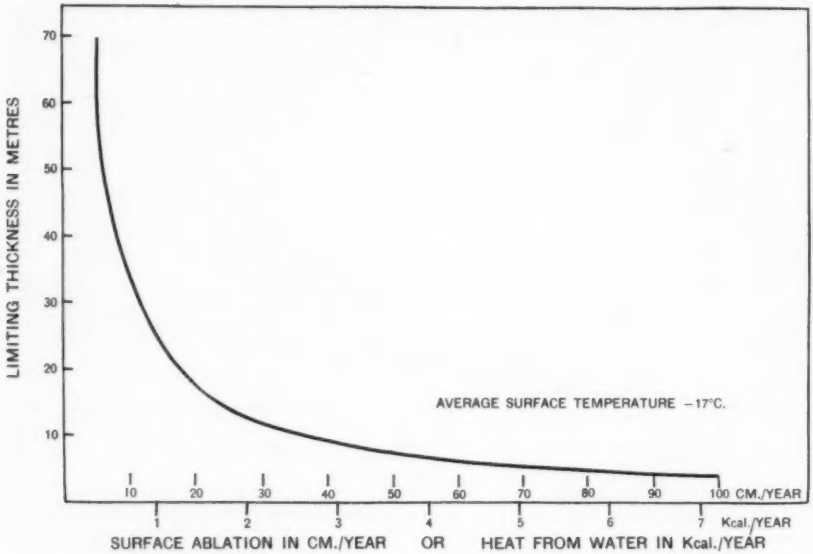

Fig. 3. Limiting values of ice thickness associated with varying values of surface ablation or heat contributed by the ocean waters.

expects that further analysis of his data will show that radiative heat is the most important element in the summer thermal budget. Fritz (1958), analysing incoming radiation data taken on T-3 in the summer of 1953, suggests that upward heat transfer by turbulence may be a large factor in accounting for small amounts lost in melting. Of particular importance in studies of the thermal budget in the Arctic are the effects of the cloud cover, which is characteristic of the summer in the high Arctic.

As noted above, the heat contributed by arctic waters also limits the growth of arctic ice. This heat is supplied by the Atlantic waters entering the Arctic through the Greenland Sea. Although oceanographic stations are still scarce, observations on ice-floe and ice-island stations in recent years have increased the knowledge of these Atlantic waters considerably. Timofeev in recent articles (1957, 1958) has given figures for water budget, Atlantic water circulation, and heat budget. Both Timofeev and Worthington (1953), though their conjectures of circulation in the Beaufort Sea areas differ, have shown an outflow of Atlantic waters in the western part of the Greenland Sea. Nansen (1902), Sverdrup (1956), and Shirshov (1944) originally considered the Atlantic flow only a compensating current for the excess of loss of surface ice and water in the Greenland Sea over contributions from Pacific waters and continental run-off.

Timofeev's values for the annual heat budget balance of the waters of the arctic basin, based on observations at many of the recent drifting

Table 1. Mean snow depth at stations in the Canadian Arctic Archipelago. (Data from Meteorological Branch, Canada Department of Transport).

Station	*Location*	*Years of record*	*Snow depth in cm.*											
			Jan.	*Feb.*	*Mar.*	*Apr.*	*May*	*June*	*July*	*Aug.*	*Sept.*	*Oct.*	*Nov.*	*Dec.*
Alert	82°30′N. 62°20′W.	5	36.3	38.6	37.3	32.5	27.2	13.2	1.3	2.8	14.7	26.7	30.0	33.0
Eureka	80°00′N. 85°56′W.	8	14.5	15.7	17.8	19.8	19.6	9.6	0.5	0.0	2.3	5.8	9.1	12.7
Isachsen	78°47′N. 103°32′W.	7	18.8	18.3	18.8	21.6	21.8	11.7	1.3	0.5	4.8	11.2	15.0	17.0
Mould Bay	76°16′N. 119°28′W.	7	15.5	17.0	19.3	22.1	22.6	12.2	1.3	0.5	4.3	9.1	11.7	13.7
Resolute	74°41′N. 94°54′W.	8	21.1	23.4	24.8	26.9	31.2	18.8	1.5	0.2	4.5	13.0	18.5	19.6

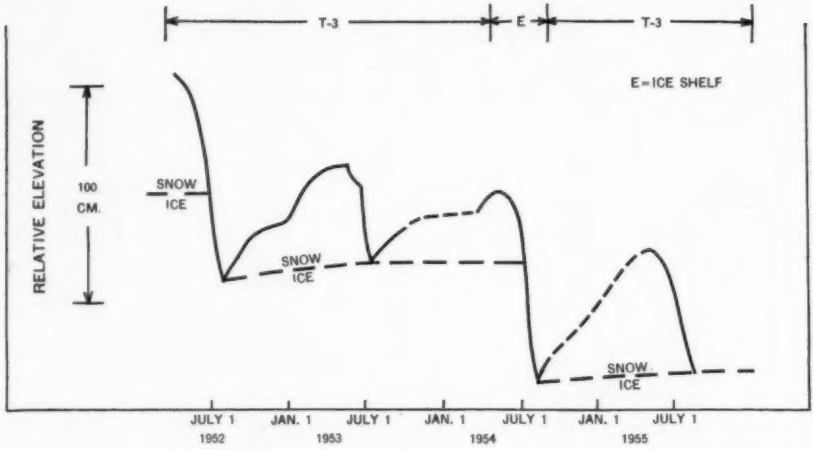

Fig. 4. Accumulation and ablation on T-3 and the Ward Hunt Ice Shelf.

stations, are given in Table 2. Although these figures are very speculative they probably represent as good an estimate as is possible at this time.

If the heat expended in the central arctic basin were distributed evenly over the approximately five million square kilometres of the arctic basin, each square centimetre would receive about 3400 calories annually, which, without surface changes, would restrict ice thickness to about 7 metres (*see* Fig. 3).

Fig. 5, constructed from Timofeev's values (Timofeev 1957), with added information obtained at T-3, shows the total heat above 0°C. in a vertical column of water of 1 cm.^{2} cross section and illustrates how the supply of heat decreases with increasing distance from the source. From this chart the annual heat loss in a given area could be estimated if the speed of the Atlantic water current were known.

Table 2. Annual heat balance of arctic basin waters. (from Timofeev 1958).

	Incoming heat (kcal.)
Atlantic waters (98,082 km.^{2})	214,357 x 10^{12}
Freezing of ice (3,108 km.^{3})	248,640 x 10^{12}
Pacific waters (36,000 km.^{3})	33,490 x 10^{12}
Continental run-off (2,377 km.^{3})	10,987 x 10^{12}
	507,474 x 10^{12}
	Outgoing heat (kcal.)
Atlantic waters	72,881 x 10^{12}
Lost to atmosphere in peripheral seas (assumed)	263,070 x 10^{12}
Lost to atmosphere in central basin	171,523 x 10^{12}

Standard methods of obtaining velocity by calculation of dynamic heights are not yet applicable in the arctic basin because of the scarcity of synoptic information. The T-3 data are of little use as the ice island apparently moved along flow lines providing no areal distribution of data from which velocities could be calculated.

Timofeev (1957) has calculated the average life of Atlantic waters to be 5.9 years by using the ratio of total kcal. (above 0°C.) in the arctic basin to the heat brought in annually. It would seem more logical to use the ratio of volumes, when, using Timofeev's figures, the life would be 28.1 years.

From known rates of ice drift it would take about 4 years for surface ice and water to move from the Alaskan coast to the Greenland Sea. The approximate time for the Atlantic waters to make this trip in the reverse direction should depend on this time and the ratio of volumes of water involved. If as Shirshov believes (Shirshov 1944) the lower boundary of the upper waters can be taken at the level of maximum vertical stability, where maximum increase in salinity is found, the surface ice and water

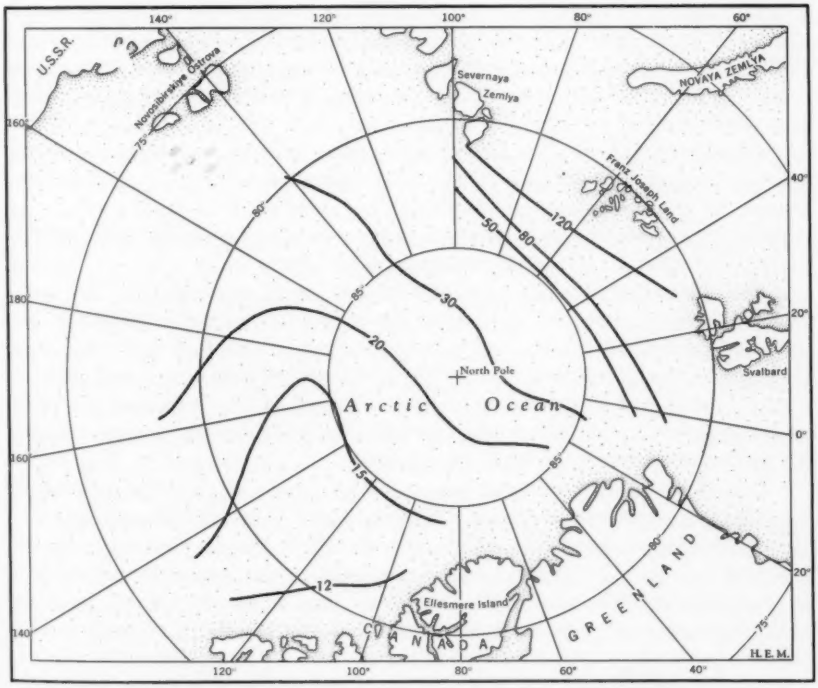

Fig. 5. Lines of equal heat. Values are total heat, in Kcal., above 0°C. in a vertical column one centimetre square.

would extend to 75 metres. If the Atlantic waters, on the basis of temperatures, occur from 200 to 600 metres the movement of Atlantic waters across the arctic basin should take about 20 years.

In an attempt to determine the age of Atlantic waters a sample taken at 400 metres at 82° 45′N., 98°30′W. was analyzed for tritium content by Giletti and Kulp (1959) and estimated to be less than 3 years old. The author speculated that the source of this water may have been the Canadian Archipelago, but Barnes and Coachman (1959) consider this unlikely and that results are too sketchy to serve as a basis for determining water circulation. The importance of analyzing further samples for tritium concentration is very apparent.

The above arguments mainly indicate how little is known of arctic water circulation. If the total life of Atlantic waters were 20 years, velocities would average about 150 km./year. According to Fig. 5 this would mean a loss of 2.0 kcal./year/cm.^{2} from Atlantic waters. If this heat flow were all lost through the ice it would restrict the ice growth to 12 metres according to Fig. 3.

It is difficult to estimate the differences, if any, between the factors governing the growth of ice in the Arctic Ocean and along the northern shores of Ellesmere Island and Greenland. As noted in the Ellesmere Island studies (Crary 1956) Atlantic waters were found off the ice shelf and also in Disraeli Bay. In Fig. 5 it can be seen, however, that the Atlantic waters have lost much of their heat before arriving at the shores of the western part of the Arctic.

A possible deterrent to the growth of thick ice near the shores might be the summer run-off, but this is not extensive in northern Ellesmere Island. At present the melt water does not extend to ice shelf depths and appears to expend its small amount of heat in melting the thin ice on the "moat" along the landward edge of the shelf.

The dissipation of heat of the Atlantic water upward must be quite variable both in time and space and dependent on factors influencing the eddy conductivity at depth. In this respect the movement of pack ice along the edge of the ice shelves in northern Ellesmere Island and the flow of tides in and out of the bays should have some effect on increasing the eddy conductivity values, which would normally be quite low in the ice-covered Arctic Ocean as compared with other oceans.

One of the most important factors in the continual growth of thick ice is its drift. The main arctic pack ice is constantly drifting, with a maximum life expectancy of about 4 years for any ice floe in the eastern arctic basin. The drift rate of T-3 indicates that ice formed in the seas north of Alaska or near the Canadian islands can survive much longer, but under present drift patterns thawing in the more southern waters during the cycle would probably prevent growth of abnormally thick ice.

The evidence from T-3, the Ellesmere ice shelves, and the ice fields near the ice shelves shows that as much as 30 or 40 metres of ice has been built up at the upper surface some time in the past. The evidence from

the T-3 dirt layers shows that this was a slow process with many periods in which the summers must have been quite similar to that of 1955. The heavy dirt layers below this accumulated ice may represent surface material consolidated after other long periods of ice accumulation. From whatever meteorological cause, this condition must have been representative of all high latitude areas of the Arctic Ocean. As the ice becomes thicker its breaking strength increases and hummocking decreases. The movement will become less on account of decreased wind stresses and increased Coriolis effect from added thickness. It will also deviate more to the right of the wind (Browne and Crary 1958). This condition would again be reversed by changing atmospheric factors. Landfast ice along the northern coasts of Ellesmere Island and Greenland, retained by protecting bays and other coastline configurations, can have an indefinite growth period.

Probable history of ice shelves

Collections of material for age determinations by the carbon-14 method from T-3 and Ellesmere Island included dirt layers, driftwood, and marine shells. As was mentioned in Part I the dirt layers on T-3 showed minute amounts of carbon material, mainly plant remains, under microscopic examination. To obtain the necessary amount of carbon for age determination 2 or 3 kilograms of the dirt material were needed. All samples were analyzed by Broeckner, Kulp, and Tucek (1956). Information on the various samples from T-3 is given below.

(1) Sample 192B. Surface dirt from Area A (Part I, Fig. 6), collected early in 1952. Two different countings of this sample gave 5720 $\pm$ 250 years and 5830 $\pm$ 200 years. This may not represent an average age of all dirt.

(2) Sample 192A. This was laboriously collected along the shallow drilling section A (Part I, Fig. 19), from a lower dirt layer. About 10 cubic metres of ice were excavated, giving 365 grams of dirt and only about 1 gram of carbon, considerably less than needed for an accurate age determination. The ages that were obtained in two different counts were 4370 $\pm$ 200 years and 4480 $\pm$ 200 years. Although this sample should represent a very definite depositional period in the island's history, the evidence is open to some doubt since 38 separate meltings of about 3 to 4 hours each were required to melt the ice, which could have easily been contaminated by the soot from the diesel fuel used in the stoves.

(3) Sample 192E was collected in 1952 in Area B (Part I, Fig. 6), and consisted of small pieces of plant material. The age was determined as 450 $\pm$ 150 years. This is not necessarily representative of the top dirt layer as the material was probably washed down on the island from a nearby land mass. However, it does represent a maximum for the time since the island broke away from land.

(4) Sample 213D. After Marshall had located the surface outcropping of the deep heavy dirt layer found in the camp-site drill holes in 1953 a pit excavation was made in Area C and 3 to 4 kilograms of material obtained.

This layer seems identical with the layer at the camp site, but as it was obtained about a half metre below the top layer it may or may not include intervening dirt layers that were found at the camp site. Age figures on two counts were 3050 ± 150 years and 3100 ± 180 years.

(5) Sample 298B. In 1955 a further excavation was made to the heavy bottom layer in Area A where it was separated from the top layer by only about half a metre. The age of this sample was 3000 ± 200 years, a very good check on 213D from the opposite end of the island.

(6) Sample 298A. In the area of 298B the dirt from the top layer was also obtained. The age for this sample was 3500 years.

The information given by these age determinations is quite conflicting. Generally the top surface dirt layer is older than the bottom layers, although 192E may perhaps be discounted because of possible contamination and shortage of carbon material. The carbon available has been shown to be of microscopic size and it might be questioned whether this wind-blown material has an age contemporary with the period at which it was blown on the ice. The most reliable samples, and those least susceptible to contamination are the bottom layers 213D and 298B.

During 1953 and 1954 considerable material, mainly pieces of driftwood, was collected by Hattersley-Smith and members of his Ellesmere Island Expeditions and ages were determined by carbon-14 analysis. The sources of the samples are shown in Fig. 6 and the data are summarized in Table 3.

The pelecypod and mollusc shells noted in Table 3 were found at 38 metres and at 60 metres above sea-level respectively, giving an average rate of the rise of land with respect to sea-level of about 0.5 and 0.8 metres per century. The presence of these shells also indicates the absence of any major glacial advance in the past 7200 years in this area.

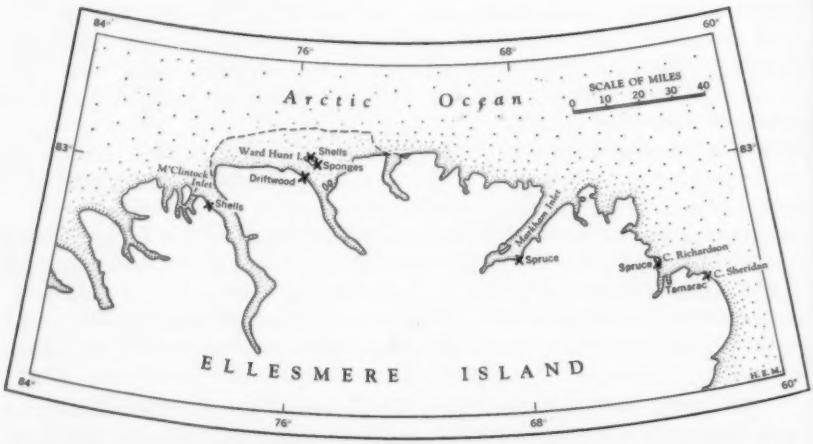

Fig. 6. Map showing locations where samples were obtained for radiocarbon dating.

Table 3. Natural radiocarbon measurements.

Sample No.	*Description*	*Apparent age (radiocarbon years)*
L261A	large fragment of spruce found south of Cape Richardson in mudbank above stream, 6 metres above sea-level	980±100
L261B	small fragment of spruce from low point on east side of Markham Inlet found in glacial outwash and raised beach material, 7 metres above sea-level	2190±150
L261C	small fragments of tamarack (?) from raised beach above river valley at Cape Sheridan 30 metres above sea-level	6050±200
L254A	wood from northern shore of Ellesmere Island, inland from ice shelf, not more than 3 metres above sea-level	3400±150
L254B	more wood from same general area as L254A	5740±200
L254C	more wood from same general area as L254A	6120±150
L254D	more wood from same general area as L254A	3000±200
L248A	pelecypod shells from raised beach on north side of Ward Hunt Island, 38 metres above sea-level (surveyed by theodolite)	7200±200
L248B	mollusk shells from west side of M'Clintock Bay among undisturbed marine deposits near glacier face, approximately 60 metres above sea-level	7200±250
L284	sponge found on surface of ice shelf near east end of Ward Hunt Island	400±150

Sample 284 was a large sponge found on the ice surface near Ward Hunt Island. Apparently the ice has thickened or the land has risen sufficiently in the last 400 years to ground the ice and by surface thawing and bottom freezing, in the manner described by Debenham (1954), the sponge was brought to the surface. This would require a net ablation in the last 400 years of an amount at least equivalent to the ice thickness in that area.

The driftwood pieces located back of the present Ellesmere ice shelf with ages varying from 3000 to 6120 years could not have arrived at their positions in the presence of the ice shelf and therefore give a maximum age for the beginning of the ice shelf. Although this driftwood could most easily have been brought in during a period of an open or nearly open Arctic Ocean, sample 261A with an age of only 980 years was most likely deposited when the Arctic Ocean was ice-covered.

No direct evidence for the minimum age of the ice shelf is available. On T-3 the best indications of minimum age come from the complexity and amounts of the dirt layers. The smallest ones, presumably the result of a single melt season, consist generally of scattered grains or very thin layers, which could not easily be weighed. The majority of layers showed evidence of having experienced several years of melting at the surface and though they could generally be followed for short distances in a

section, it was necessary to core holes half a metre apart in order to trace many of them. The layers in the deep holes, all drilled in the same general area, could not be correlated, except for the heavy bottom layers. An age determination based on the evidence of probable annual deposition and total dirt would be several thousand years.

Further evidence leading to an estimated age is the uplift of the land areas. The outcropping of a heavy dirt layer on the edges of T-3, 10 to 15 metres higher than its location at the camp site on T-3 and the steep dip of the ice layers south of the Ward Hunt ice rise (Marshall 1955) are best explained by a rising of land areas. The old strand cracks found at an elevation of more than 20 metres above the present active ones on the south side of the Ward Hunt ice rise are difficult to explain except by uplift of the land. Using the carbon-14 age of the marine shell samples, which give an uplift of about 0.5 metres per century, a minimum age of a few thousand years could be expected for both ice island and ice shelf.

At two sites near the edge of T-3 outcrops of the heavy dirt layer are attributed to the uplift of land along the coast of northern Ellesmere Island, where these parts of the island were attached to the shore originally. At Rocky Point much morainal material, rocks, and some glacier ice were found. At Colby Bay considerable plant and animal material was obtained, including fish remains and *Serpula* tubes, which must have been picked up from the bottom of the water by the freezing ice. From geological evidence the most likely sites for the origin of T-3 appear to be Cape Bourne or Yelverton Bay. An original location deep in Yelverton Bay, as shown in Fig. 7, would account for the uplift at the two opposite ends and for the material found in the Colby Bay area.

Even though the general structures of T-3 and the present ice shelf in the Ward Hunt Island area are similar, the evidence of several age determinations indicates that the two may not be contemporaneous, and that T-3 may be older than the ice shelf in the Ward Hunt Island area. If we accept the ages of the ice shelf and ice island as greater than that of the driftwood samples L261A and L261B (980 and 2190 years, respectively), found a considerable distance east of the ice shelf, then these samples must have been brought in during periods of an ice-covered Arctic Ocean not unlike the present, as the ice shelf could hardly have existed in the presence of an open Arctic Ocean. Similarly, at an earlier period, when the T-3 ice island was in Yelverton Bay, but prior to the growth of the present ice shelf, driftwood could have been deposited on the mainland south of Ward Hunt Island by moving pack ice. However, if the age of the ice island can be shown from future evidence to be less than that of the driftwood, the possibility of an open Arctic Ocean during deposition of the samples south of Ward Hunt Island, and thus before the growth of the shelf, would be increased.

From evidence on T-3 and assuming that the wind-blown dirt particles were deposited uniformly with time, the last 7 per cent have been deposited during an ablation period and about 22 per cent before that were deposited

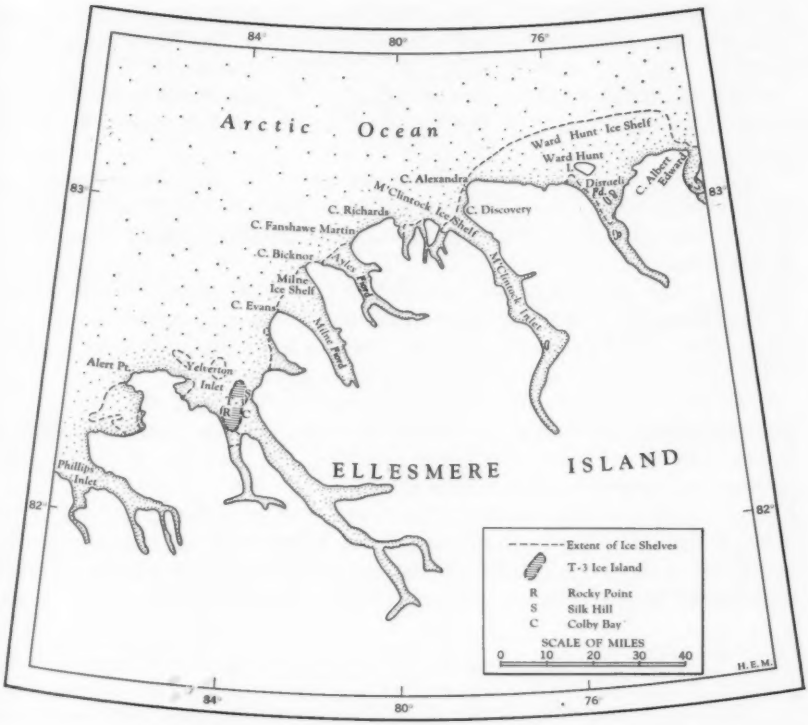

Fig. 7. Map showing possible source location of T-3 in Yelverton Inlet.

during a general accumulation period in which the island may have grown by about 20 to 30 metres. Any evidence of accumulation of snow older than the main dirt layer has been lost by later ablation. If an average age for the lower dirt layer is taken as 3000 years, this would give about 5500 years for the oldest dirt in this layer. Assuming that the ice of the island started to build up about 5500 years ago, the last accumulation period would have begun about 1600 years ago and continued until about 385 years ago, when the present ablation period started.

An ablation period starting about 400 years ago is substantiated by the sponge found on the ice surface, with an age of 400 years, and by the plant material, Sample 192E, 450 years old, also located on the surface. Surprisingly, evidence for this ablation period was found only in a few localities near the Ward Hunt ice rise, where old strand cracks existed mainly on the grounded ice and are believed to have resulted from uplift of the land. Where the underlying land is dipping very gently, horizontal changes in locations of strandlines would be greatest. The air photographs

of the Ellesmere ice shelf very plainly show evidence of older, more extensive grounded ice in the northwestern part of the Ward Hunt Island ice rise.

The historical evidence that pertains to climatic changes and that has resulted from the study of the ice islands and ice shelves can be stated briefly:

(1) From carbon-14 dates of the dirt from T-3 and the driftwood on the landward side of the Ellesmere ice shelf it can be concluded that these features are definitely later than the end of the Wisconsin ice age some 11,000 years ago.

(2) Thick growths of ice, of which T-3 is a remnant, probably began in Yelverton Bay about 5500 years ago, and in the vicinity of Ward Hunt Island about 3000 years ago. As part of the inner ice in Yelverton Bay, T-3 was protected from the movement of arctic pack ice.

(3) Along the shores of northern Ellesmere Island the deposits of marine shells indicate that there has been no general advance of the glaciers in the area for at least 7200 years, and that the land has risen 38 to 60 metres with respect to sea-level in this time.

(4) Evidence from T-3 dirt layers and from material that worked up through the ice indicates that the present ablation period has been going on for about 400 years and that it was preceded by an accumulation period about three times as long.

Discussion of surface features

The major surface features that characterize the ice shelves of the Arctic are ridge and trough systems of quite uniform wave-length. Along the survey line on the ice shelf near Ward Hunt Island the average distance between ridges is 235 metres, and measurements from T-3 (Fig. 2, Part I) give values varying from 230 to 260 metres. This is a close agreement considering that the ice island T-3, as discussed above, probably had its origin about 200 kilometres southwest of the shelf. Depths of the troughs vary from a fraction of a metre to 6 metres. The cause of the uniformity in wave-length must be a factor that is itself uniform in high arctic areas.

Examples of the effect of stresses on floating ice shelves can be found in both arctic and antarctic areas. On newly formed sea ice a few centimetres thick that is subjected to shearing stresses insufficient to buckle the ice completely, *en échelon* systems of anticlines will occur at approximately 45° to the direction of the stresses. However, these are quite exceptional and anticlinal systems on arctic ice floes subjected to almost continuous stresses are significantly absent. In Antarctica many types of deformation features are formed by stresses in floating ice. Best known are those in the Ross Ice Shelf caused by the movement of ice around Roosevelt Island. Compressional forces north of the island causing the "Gould" anticlines have been studied by Zumberge (1958). Valleys and rifts are common in the tension areas along the front of the Ross Ice Shelf

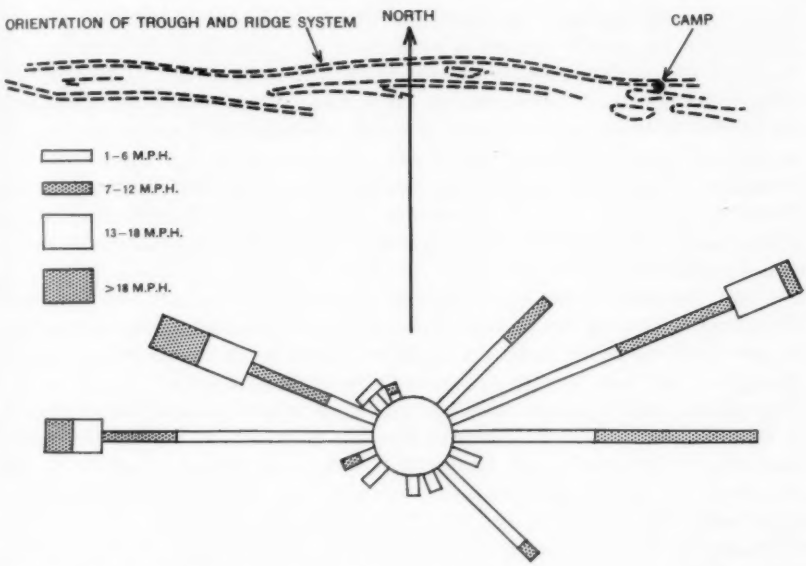

Fig. 8. Sketch showing orientation of ridge-and-trough system compared with wind rose.

on either side of Roosevelt Island. In both cases they are the result of the slow steady forces of moving ice, factors now absent in the shelf ice of northern Ellesmere Island. Despite the presence of Ward Hunt Island and other submerged islands no deformation features such as would be expected if the ice were moving can be found. In particular, if glaciers from land areas were a major factor in the formation of the shelves, it would be expected that some surface evidence of glacier action would remain, but except for features at opposite ends of T-3 they are absent. Any possibility that the ridges may have developed by grounding along submerged features of the shore must be discarded because of lack of uniform water depths and seaward motion.

It is believed, as suggested by Hattersley-Smith (1957), that the ridge and trough systems are the result of the action of strong summer winds on melt-water lakes and ponds. Fig. 8 shows that the prevailing surface winds during the 1954 expedition to northern Ellesmere Island were parallel to the ridge and trough systems. Fresh water on ice is a very efficient medium for ablating the ice because of its lower albedo and the convection processes below 4°C. High surface winds pile up water downwind and at the same time speed up these convection currents. Any series of randomly scattered ponds should in time form elongated lake systems under the influence of a system of prevailing winds. These elongated lakes would become deeper in time, although from evidence on both T-3 and the ice shelves, the melt-water lakes appear to be seldom deeper than 1.5 metres,

a limit imposed probably by the decrease in convective currents in deeper water.

The second factor that could cause the formation of systems of elongated melt-water lakes would be surface slope. Although favoured accumulation areas could occur locally, particularly in the immediate vicinity of land, the slight slopes seen on the shelf and ice island are probably caused by the increased surface ablation on the downwind ends of melt-water lakes. Inside the fiord or inlets, such as Disraeli Bay, wind systems are complicated by surface topography and complicated ridge and trough patterns develop.

If the troughs are the product of the influence of wind systems on melt-water lakes, then the wave-lengths should depend primarily on the amount of summer melting. The problem would be much the same as that associated with the preferred size and spacing of randomly located melt-water lakes on flat arctic ice floes. Large lakes would generally be favoured as they would be more effective in causing additional melting. The sizes, however, are limited by the tendency of the ponds to grow mainly in the direction of the prevailing winds, and also by the effects of the local topography. In Fig. 3 of Part I it is shown that on many of the wide ridges of the ice shelf individual ponds are formed that should eventually grow to either elongated lakes or tributaries of larger systems. Distances between lakes should generally be comparable to lake widths.

That the trough systems migrate horizontally with time is apparent from the results of drilling on T-3 during 1955 (*see* Fig. 26, Part I), but it appears that this is a very slow process. Normally on the Ellesmere ice shelves lateral migration of ridge and trough systems might be expected from increased melting on the southern slopes of the ridges. T-3, formed in a bay area, must have been subject to other factors such as differential snow accumulation or differential melt-water flow from land.

Discussion of ice islands

As is now well known the floating ice islands are parts of the vast ice shelf areas of northern Ellesmere Island and Greenland that have broken loose under the action of meteorological or oceanographic factors. Although the term "ice island" is not wholly suitable and would certainly not find favour in Antarctica, where huge tabular floes are commonplace, the usage has found general acceptance in the Arctic Ocean, where icebergs or parts of ice shelves are rare.

Discussions of many aspects of the ice islands, their locations and probable numbers followed soon after the original discovery (Koenig *et al.* 1952, Montgomery 1952). Although over 80 have been located, most of these have been in the bays and inlets of the Canadian Arctic Archipelago, and many have undoubtedly been grounded there for gradual melting. As far as their recent history is known, only T-1, T-2, T-3, and NP 6 have been followed for any length of time in the arctic basin. The largest, T-1, may

still be in the Arctic, although its last sighting was at 80°45'N. 4.0°E. Though it is not expected that more than a few ice islands are now in the arctic basin, no thorough search has been made since 1951, when J. O. Fletcher directed systematic flights over the western half of the Arctic before the occupation of T-3.

As seen from the general circulation pattern of the surface waters in the western part of the arctic basin (Browne and Crary 1958) tracks of the known islands from northern Ellesmere Island are gyral, swinging south to the Beaufort Sea, then westward and north along approximately the 180th meridian. Of the four known ice islands two have continued past the Pole to escape from the Arctic Ocean into the Greenland Sea, and two have continued to another circuit of the western arctic basin. As far as can be estimated the possibilities of an ice island leaving the Arctic or continuing in the gyral pattern are about equal. The chance of being caught in the Canadian Arctic Archipelago must be rather greater on the first round than on the succeeding ones. A lifetime of more than 30 years in the arctic basin would be improbable under present circulation patterns. Polunin (1955a, 1955b) has estimated from comparison of growth rings of arctic willow from T-3 and Ward Hunt Island, and from observations of the moss *Hygrohypnum polare*, found on T-3, that T-3 left its original site not earlier than 1935. The difference noted between the outline of the ice shelf near Ward Hunt Island as given by Marvin in 1906 (Bushnell 1956) and that found in 1954 indicates that several parts of this shelf have broken off in the intervening period. T-1, T-2, T-3, NP 6 and possibly several other ice islands, as Crocker Land (Peary 1910) and Bradley Land (Cook 1911), represent sizeable portions of the shelf that have broken loose in the later part of the ablation period discussed above. This is rather surprising in view of the indicated age of the shelf and island. It would appear that we are indeed approaching a period when the ice shelves might very well become extinct. It can be inferred from this that at present the arctic areas must be as open and the ice as thin as at any time in the last 3000 years.

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NOTES

THE AGE OF THE METAMORPHIC COMPLEX OF NORTHERNMOST ELLESMERE ISLAND*

Among the geological results of the Nares Expedition of 1875-76 to northern Ellesmere Island was the mapping of an area of mica schists and other altered rocks between Stubbs Point and Markham Inlet[1], and in the same general area Peary[2] noted the presence of igneous rocks. These observations led Schuchert[3] to postulate a borderland of Archaean rocks, the supposed source of the sediments deposited in the Franklinian Geosyncline, to which he gave the name "Pearyea".

Recently the Precambrian age of these rocks has been questioned and the view has been expressed that the metamorphic rocks of northern Ellesmere Island are merely highly metamorphosed Palaeozoic strata. Support for this view comes from north Greenland where it appears that Palaeozoic beds can be traced into regions of metamorphic strata.

In 1953 the writer[4] mapped a group of migmatites and gneisses between Cape Aldrich and Markham Inlet and named them the Cape Columbia group. Christie[5] continued geological mapping in the area during the 1954 field season and extended the group to include all rocks of advanced metamorphic grade. He mapped outcrops of the Cape Columbia group between Cape Aldrich and Cape Albert Edward, on Ward Hunt Island, at the head of M'Clintock Inlet, and between Ayles Fiord and Phillips Inlet. He also found a conglomerate containing pebbles of Cape Columbia group rocks in what he named the M'Clintock group; on other evidence the M'Clintock group was shown to be older than the Middle Ordovician. Christie also found fragments of metamorphic rocks in a fossiliferous conglomerate in the Challenger group of Upper Ordovician age. Thus from geological evidence it was possible to state with some confidence that the age of the Cape Columbia group was pre-Ordovician.

A specimen of biotite-rich gneiss was recently submitted by the writer to the Isotope and Nuclear Research Section of the Geological Survey of Canada for age determination using the potassium-argon method, which gave an age of 545 million years. The significance of this result in terms of geochronology is discussed in the remaining part of this note.

The geological time scale has been subject to some revision in recent years. Kulp[6] in an article on isotopic dating and the geologic time scale indicated that at that time, late Precambrian time was thought to extend from 700-500 million years ago. However, in 1955 the potassium-argon technique was still being developed and the time scale in use was based mainly on lead isotope-ratio methods. Recent papers show a considerable divergence of opinion regarding the date of the base of the Cambrian period. Mayne, Lambert, and York[7] suggested that the time scale be expanded, and they placed the middle of Upper Cambrian time at 650 million years later rather than at 450 million years as proposed by Holmes[8]. Kulp, *et al*[9] in replying to this paper pointed out erroneous assumptions made by the Oxford group and suggested that the Kolm, in the Upper Cambrian black shales of Sweden, has a minimum age of about 500 million years. Davidson[10] presents a table listing results from various investigating groups; his data for the Cambrian are presented below.

Mid-point of the Cambrian period (m.y.)

Holmes	Belousov	Oxford	Recent Experiments
470	423	700	513

* Published by permission of the Director, Geological Survey of Canada.

It seems obvious that investigations now in progress in a number of laboratories of the age of stratigraphically well-defined rocks will soon permit a much better definition of the time scale.

If we accept the consensus of current thought, it appears probable that the last metamorphism to which Cape Columbia group rocks were subjected occurred in lowermost Palaeozoic time or uppermost Precambrian time; the rocks themselves may be much older as the method dates only the most recent metamorphism.

The existence of metamorphosed strata in northern Ellesmere Island suggests that orogenic forces may have been involved and the resulting landmass may have been the source of the clastic sediments that Thorsteinsson and Tozer[11] note in the Parry Islands and Ellesmere Island. By the close of the Palaeozoic era, the area occupied by the Cape Columbia group rocks had been lowered and limestone of Permian age was being deposited with angular unconformity on the gneissic and other metamorphic rocks of the Cape Columbia group.

R. G. BLACKADAR

[1]Feilden, H. W. and C. E. De Rance, 1878. Geology of the coasts of the arctic lands visited by the late British expedition under Captain Sir George Nares. Quart. J. Geol. Soc. 34:556.

[2]Peary, R. E. 1907. Nearest the Pole. London: Hutchison and Co., pp. 213, 231.

[3]Schuchert, C. 1923. Sites and natures of North American geosynclines. Bull. Geol. Soc. Am. 34:151-230.

[4]Blackadar, R. G. 1953. Geological reconnaissance, north coast of Ellesmere Island, N.W.T. Geol. Surv. Can. Pap. 53-10.

[5]Christie, R. L. 1957. Geological reconnaissance of the north coast of Ellesmere Island, N.W.T. Geol. Surv. Can. Pap. 56-9.

[6]Kulp, J. L. 1955. Isotopic dating and the geological time scale. Geol. Soc. Am. Sq. Pap. 62:609-30.

[7]Mayne, K. I., R. St. J. Lambert, and D. York. 1959. The geological time scale. Nature 183:212-14.

[8]Holmes A. 1947. The construction of a geological time scale. Trans. Geol. Soc. Glasgow. 21:117-52.

[9]Kulp, J. L., J. C. Cobb, L. E. Long, and D. S. Miller. 1959. The geological time scale. Nature 184:BA62-3.

[10]Davidson, C. F. 1959. The geological time scale. Nature 184:1310.

[11]Thorsteinsson, R. and E. T. Tozer. 1960. Structural history of the Canadian Arctic Archipelago since Precambrian time. Geol. Surv. Can. Pap. 60-7.

ANDREW GRAHAM, THOMAS HUTCHINS, AND THE FIRST RECORD OF PEARY'S CARIBOU

The eighteenth century naturalist Thomas Pennant published the following note in 1787:

> "Mr. *Hutchins* was presented, by the *Weahipouk Indians*, with a Deer four feet eight inches long and three feet two high. It was entirely white, except for the back which was mottled with brown. The fur was short and fine like that of the Ermine. The Indians, in their manner of expression, said it came from a place where there was little or no day." (Ref. 2, p. 51).

This description sounds extremely like Peary's caribou, but there are obvious difficulties in accepting it as the first record of that remote species of deer. Who, it must be asked, were the "Weahipouk" Indians? How were any Indians able to secure the skin of an animal that lived north of the range of most Eskimos? And how did they come to make a present of it to Dr. Thomas Hutchins who spent most of his time in America at Fort Albany on James Bay and was never at any time north of Fort York? (Ref. 4, p. 263, note 1).

Through the kindness of Miss Alice Johnson, archivist of the Hudson's Bay Company, it has become possible to answer these questions and also to assert that Pennant's note is indeed the earliest description of Peary's caribou, although an inaccurate one.

The archives of the Hudson's Bay Company possess twelve manuscript volumes entitled *Observations on Hudson's Bay*. The three earliest are by

James Isham and date from the 1740's[3]. One of the others is ascribed on the cover to Thomas Hutchins and has often been quoted (e.g., by Seton) as his work. It is in fact not Hutchin's work. It contains observations from Prince of Wales's Fort at Churchill, Man., which Hutchins never visited, and its text is consistently similar to that of eight other manuscript volumes of the *Observations on Hudson's Bay* that were written or rewritten by one Andrew Graham, who was chief of the Hudson's Bay Company posts at Severn, York (1772-3), and Churchill (1773-5). To Graham must thus be ascribed this, as well as the other manuscripts. In three of Graham's manuscripts Pennant's deer skin is described in almost identical words as follows:

> "Wappew-Tick the White Deer. This species is five feet eight inches long [not four feet eight inches, as Pennant wrote]; Three feet two inches broad ["broad", not "high", as Pennant wrote]; legs one foot nine inches long [omitted by Pennant]; the hoof is black, two inches long and *one and* a quarter broad [omitted by Pennant]. It is quite white and does not change colour in the summer time like the Hares, but continues the same in all seasons; except when old the back is mottled with brown hair intermixed with the white. The furr quite short and much resembling the Ermine; the Esquimeaux who I am now conversing with says they very seldom see any of them. The Wechepowuck [note Pennant's misspelling] Indian who brought me this skin says they harbour where very little or no day light appears. I have strictly examined the southern Natives [the Crees, who traded at Churchill], the oldest of whom never did see or hear tell of such coloured deer. I presented the skin to the Directors of the Hudson's Bay Company."

This is the text in the volume catalogued as Hudson's Bay Archives E. 2/12, p. 37; the words in italics are added from the text of the other two manuscripts, Hudson's Bay Archives E.2/10 and E.2/13. The last of these three manuscripts, E.2/13, is the one wrongly attributed to Hutchins and contains the following further information:

> "Last Summer I had in to Trade nigh three hundred Wechepouck Indians headed by the leader who conducted Mr. Hearne to the Coppermine river, from him I had several pieces of Copper, also two Deer skins which are entirely white and the hair firm in the Pelt and nearly as fine as Ermine furr."

The Indian who provided the deer skins is identified as Hearne's guide, Matonabbee. This identification establishes his fellow "Wechepouck Indians" as Chipewyans, for "he was made head of all the Northern Indian nation" on his return to Churchill in 1772 (Ref. 1, p. 227). The post where the skins were received must have been Churchill, since Eskimos are described as being there and the Chipewyans traded regularly there, whereas neither of these people normally came as far south as Fort York. The date is obviously *after* Hearne's journey and must have been 1774 or 1775 as these were the only two summers in which Graham had charge of Churchill. Since Matonabbee brought Graham "several pieces of copper", it would appear that he had again been to the Coppermine River, for which he had had ample time between his departure from Churchill in 1772 and the summer of 1774. This last point makes Matonabbee's possession of two skins of Peary's caribou perfectly comprehensible. One has only to suppose that he secured them from Eskimos at the Coppermine River, whom he had helped rob in war in July 1771, and with whom he had traded peaceably on a previous visit (Ref. 1, p. 224). The Eskimos themselves could well have obtained the skins from Victoria Island, where the caribou today intergrade with the Barren Ground species, but still show strong *pearyi* characteristics. Matonabbee's description of the habitat of these deer as a land "where very

little or no day light appears" fits Victoria Island for nearly half the year and could be information derived from Eskimos who visited the Coppermine River in summer. This is quite possible because Matonabbee himself was very peaceably inclined toward the Eskimos (Ref. 1, p. 224). The assertion that the deer concerned "does not change colour in the summer time like the Hares" still more decidedly suggests sound information obtained from Eskimos. Finally, Graham's original and ungarbled description of the skin fits *pearyi* even better than Pennant's printed version.

One can therefore conclude that Peary's caribou ought to have been known to science from 1787 on, if only Hutchins and Pennant had been at pains to copy Graham's account fully and correctly.

Here again Dr. Hutchins shows his singular talent for stealing the credit due to others. This he had done earlier for all the natural history specimens sent home to the Royal Society by Humphrey Marten from Fort Albany (Ref. 1, p. 287). As noted above, Graham deserves the whole credit for the natural history observations ascribed posthumously to Hutchins by Richardson, Seton, and others, and Hutchins is here found to have imposed on Pennant too. He had ample opportunities to do this because he was the Hudson's Bay Company's corresponding secretary in London after his return from Fort Albany in 1782. His only real services to knowledge of the Arctic would appear to be the positive one of making the "Observations on the Congelation of Mercury" published in the *Proceedings of the Royal Society*, and the negative one of taking from the Hudson's Bay Company archives Hearne's Chipewyan vocabulary "containing sixteen folio pages", so that it was lost with his papers on his death in 1790 (Ref. 1, p. lii) and thus preventing it from being used by fur traders, explorers, missionaries, anthropologists and others, even if Hutchin's intention had been to have it copied.

It is high time that Andrew Graham received his due and that scientists were directed to his manuscripts, which may well prove worthy of publication. They contain much of interest to anthropologists, ornithologists, mammalogists, as well as fur-trade historians.

My thanks are due to Dr. A. W. F. Banfield for information on the caribou of Victoria Island.

RICHARD GLOVER

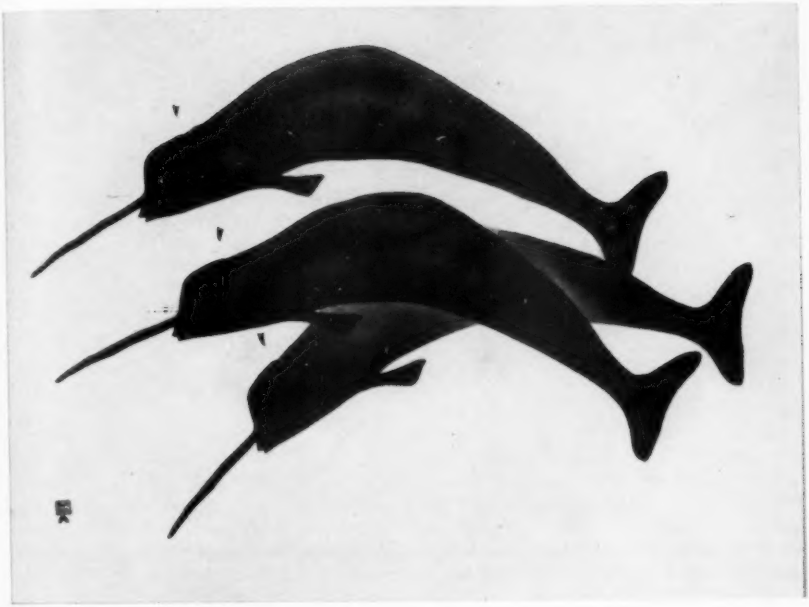
[1]Hearne, S. 1958. Journey to the northern ocean. *Ed. by* Richard Glover. Toronto: Macmillan Co. of Canada.

[2]Pennant, Thos. 1787. Supplement to the arctic zoology. London: Henry Hughs.

[3]Rich, E. E. 1949. Isham's observations and notes. Toronto: The Champlain Society

[4]Tyrrell, J. B. 1934. Journals of Hearne and Turnor. Toronto: The Champlain Society.

"Girl with Skin Line" *by Kunu (Stonecut 9 by 12 inches).*



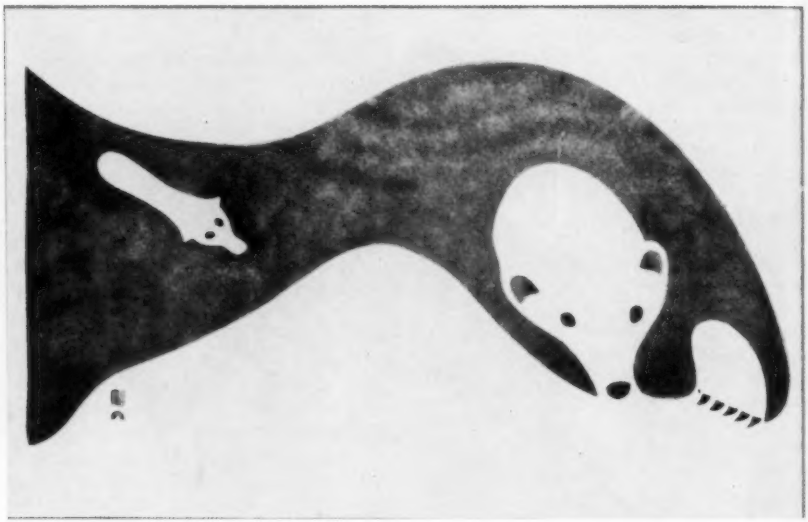
"Three Narwhales" *by Kananginak (Sealskin stencil 18 by 24 inches).*

Note on cover picture

Eskimo art in the form of soapstone carvings has been known generally for only a few years, but became popular quickly. The best of these carvings are made at Cape Dorset, a small settlement on the south coast of Baffin Island. From there now comes a new art form — stonecut prints and rubbings and prints made with sealskin stencils, the last a development of the appliqué work long used by the Eskimos to adorn their parkas.

The cover picture and the pictures accompanying this note are reproduced with the kind permission of the Industrial Division, Department of Northern Affairs, 150 Kent Street, Ottawa, Ont. A folder — "Eskimo Graphic Art" — containing 40 reproductions of prints, has been issued by the Industrial Division. From the text accompanying this

"Snowy Owls and Egg" by Iyola *(Stonecut 6 by 18 inches).*



"Polar Bear and Cub in Ice" *by Niviaksiak (Sealskin stencil 10½ by 20 inches).*

folder the following information about the three artists represented here has been taken.

Kananginak was born in 1935. He is married and has two children. He is not only an excellent print maker, but also an excellent carver. As member of an official Canadian party he represented the Cape Dorset Eskimos on a visit to Greenland a few years ago and is now president of the West Baffin Eskimo Co-operative.

Niviaksiak died in 1959 at the age of 39. He was married and had five children. Niviaksiak was not only an outstanding artist, but was also known among his fellow Eskimos as a man who deeply pondered many matters.

Kunu, Niviaksiak's widow, was born in 1923. She has had no schooling and is now in hospital for the second time.

Iyola was born in 1933, is married and has two children. He produced the print "Canada Geese in Flight" that was presented to the Queen during her visit to Canada in 1959. Iyola is also an excellent carver and one of the best hunters in the Cape Dorset area, where he lived all his live. He has had no schooling.

Correction to *"Note added in proof"* on page 218 of Vol. 12 of *Arctic.* This should read: the symbol for Kenai, numbered 43 in the diagram, should be a square instead of a cross; the symbol for Adack, No. 58, at 32.8° on the baseline, has been misplaced, it should be about ¼ inch above this position; the symbol for Kotzebue, No. 65, which should indicate 103 degree-days and a temperature of −21.0°, has been omitted from the diagram; (the symbol for Dutch Harbor, No. 63, is in the correct position).

REVIEWS

THE WHITE ROAD

By L. P. Kirwan. *London: Hollis and Carter. 8¾ x 5½ inches, 374 pages, frontispiece, 17 plates, 5 text maps and diagrams, 2 end-paper maps; distributed in Canada by Palm Publishers, Montreal, P.Q., $6.00.*

Published in the United States as

A HISTORY OF POLAR EXPLORATION

New York: W. W. Norton and Company. $5.95.

Mr. Kirwan, Director of the Royal Geographical Society, and a former director of the Scott Polar Research Institute, begins his history of polar exploration by disclaiming all personal experience of the Arctic and the Antarctic. Polar exploration is a subject that is open to controversy, and one that makes any reviewer prone to pedantry. It is a tribute to the author that in his factual, crammed pages, covering the whole scope of polar exploration, he makes a few errors, stirs a few controversies, shows some biases, but on the whole manages to do justice to practically every explorer who crossed the Arctic or Antarctic Circle.

Mr. Kirwan had access to much original material from the files of the Royal Geographical Society and of the Scott Polar Research Institute, and this, with his stated intent of writing on "the evolution of polar exploration in its historical and social context" has biased him towards an emphasis on the British approach to the poles. This approach adds fascinating detail to the sagas of Scott and Shackleton, and provides continuity as the author shows the motives behind British expeditions from the time of the Elizabethans to the present. At times this weighting in favour of the British endeavours leads to an unbalanced picture. Constantine Phipps gets one page; only four and a half are devoted to the great Russian expeditions in the eighteenth century. Nor is Mr. Kirwan prone to traditional understatement when the role of the Royal Navy is discussed. Much has been done recently to show the real nature of the achievements of men like M'Clintock, Ross, and Parry, but Mr. Kirwan, introducing the Royal Navy's part in arctic exploration says the following.

"But despite their stubborn adherence to traditional ways in most unsuitable conditions, despite their inadequate equipment, their ignorance of how best to live, and how best to travel in the polar regions, the achievements of these expeditions, now to be described, are among the most remarkable in polar history. At sea their supreme skill in the handling of cumbrous sailing ships, turning and twisting through the pack at the mercy of the winds and the ice, was a miracle of navigation. On land, their heroic journeys hauling, officers and Jack Tars alike, heavy sledge boats across the tumbling and shifting Arctic floes, were for generations the inspiration of British polar explorers." (p. 80).

When the fate of Franklin's crews is recalled this assessment may sound a little exaggerated.

At times Mr. Kirwan is a little cursory in his judgements ("The expedition [De Long's] was in itself a total failure", p. 187), but he discusses fully the feats of such non-British explorers as Bellingshausen, Wilkes, Nansen, and Sverdrup, and accords them full and fair credit for their discoveries and exploits.

Writing from the viewpoint of men whose ideas rather than whose actions stirred interest in the polar regions, the author sometimes spends too much time on people such as Byron, Dalrymple, Wallis, and de Bougainville. But his discussion of the influence of men like Sir Clements Markham and Alfred Harmsworth is extremely valuable, and helps to fill in the background on polar exploration. By paying less attention to details of field work and techniques, and concentrating on the driving forces behind polar exploration, the writer

presents a fascinating and original point of view on the reasons why men went where they did in the Arctic and Antarctic.

The author's literary style, however, may prove a stumbling block to the enjoyment of his book. At its best it is delightful and vivid. The sentence on page 240, describing the experience of Scott's men with sledge dogs — "At the merest touch of their inexperienced hands, it seemed, an apparently docile dog-team would be transformed into a welter of snarling animals and tangled harness, exhausting their patience and defying their most ingenious efforts to restore discipline and peace." — not only helps to explain the tragedy of the South Pole party, but also strikes a responsive chord in anyone who has ever attempted to handle sledge dogs. At times, however, the prose becomes too heavy and involved. Long, cumbersome sentences with many clauses tend to confuse the reader. On page 42 one sentence reads "Meanwhile, on land, the men of the Hudson's Bay Company, trappers and hunters, guides and voyageurs, who lived off the country and were learning from the Eskimo and the Indian how best to travel and survive, had already started, as they laid the foundations of a great industry, to push the Canadian frontiers towards the north". The writer has been badly served by his printers on occasion when punctuation marks have been missed out. Page 100 has a sentence that reads "In Montevideo Smith was tracked down by a group of American merchants who proved a good deal less sceptical of his discovery than John Miers and the British merchants in Valparaiso and Smith gives an entertaining picture of his meeting with them in a report he sent to the British Admiralty of 31st December 1821". It would be a great pity if this invaluable book were to be used only as a reference source; it deserves to be read through from cover to cover.

There are some errors — almost inevitable in a work of this scope and range. Robert Juet, mate of Hudson's ship the *Hopewell*, was not tried for mutiny (p. 34); he died before the ship reached Ireland. Barents died after falling "sicke" according to Gerrit de Veer, and not of "cold and exposure" (p. 32). Mackenzie and Hearne are described as "men of the Hudson's Bay Company" (p. 82); Mackenzie worked for the North West Company. Wilkes's ships, the *Vincennes* and the *Peacock* were not "sloops of 700 tons" (p. 130). One ship displaced 780 tons, the other 650 tons. Hall died on the *Polaris*, not "before reaching the ship" (p. 183). All the crew of the *Jeannette* did not reach the Lena estuary (p. 187); one boatload disappeared after the ship had been crushed. Brönlund's body was found in Lambert's Land, not "on the ice sheet" (p. 298). Courtauld was isolated in a tent, not in hut (p. 324). "Bob" Bartlett is described on page 259 as "the British captain of the Roosevelt" — true in a way, but not a statement that would endear the writer to a native of Newfoundland.

The most serious error of fact is found on page 257. "In July 1905 Peary, now fifty, sailed from New York City in the *Roosevelt* which, after some damage to her bows, reached Cape Colombia (sic) on the north-east coast of Grant Land within ninety miles of the advance base at Cape Hecla." One error of opinion lies in Mr. Kirwan's statement "Nor has the Canadian claim to ownership of the Arctic mainland and the islands between Greenland and 141°W. longitude ever been disputed." On p. 185 a sentence begins "Because of a branching of the warm waters of the Gulf Stream north of the Bering Strait. . . ."

Mr. Kirwan is at times vague in his details of distance and location. It would have been better to use either statute of nautical miles throughout the text rather than to mix them. Fort Conger is not "at the north-east coast of Ellesmere Island" (p. 254), though very near it.

Misprints include "1871" for "1891" as the year of Peary's journey across the Greenland icecap (p. 197), "Cape Colombia" for the northernmost point in Canada (p. 259), "Croker Land" for Peary's mythical island (p. 326), and "Canada's northwest territory" (p. 340).

There is an excellent bibliography, and the work is carefully indexed. The

map showing the Northwest Passage is outstanding, but the endpaper maps are inadequate and badly out-of-date.

The White Road as it stands has considerable merit, both as a reference book and as an exciting narrative. It throws new, interesting and unusual light on the problems of polar exploration. If Mr. Kirwan can eradicate on the occasion of preparing a second edition the annoying errors and shorten his overlong comma-strewn sentences, the result will be the best book by far on the history of polar exploration.

JIM LOTZ

DICTIONARY OF DISCOVERIES

By J. A. LANGNAS. *Preface by J. Salwyn Schapiro. New York: Philosophical Library, 1959. 9¼ x 6 inches, vi + 201 pages. $5.00.*

Mr. I. A. Langnas has undertaken a commendable task, but he has performed it badly. So many inconsistencies in dates and spellings can be found that no entry should be relied on as being perfectly factual. Whereas it is clear that the selection of whom to include in a limited space must be arbitrary, still the selections and omissions are frequently surprising. Included in the dictionary are not only explorers, but also pioneers, travelers, cartographers, empire builders, and scholars.

Professor Schapiro, who contributed the preface, should have examined the text more closely. To Professor Schapiro, making lands "known to the civilized world" is the meaning of "discovery". with the result that he judges Columbus to be the discoverer of the New World, not the Norsemen. Mr. Langnas has made no such distinction. For example, in the Carl Christian Rafn entry he speaks of a publication that "conclusively proved the discovery of America by the Norsemen, five centuries before Columbus", and the entries for Leif Ericson and Bjarne Herjulfsson also speak of "discovery" by the Norse. Anyway, few now deny that after 1000 A.D. the civilized world knew a good deal about Iceland, Greenland, and the northeastern coast of America.

The proper names of at least seven explorers (Cunninhame-Graham, Saint Isaac Joques, Thorfinn Karlsefri, Sir Francis MacClintock, Sir Robert MacClure, Baron Adolf Nordenskjöld, Joseph Bush Tyrrell; which should be Cunninghame-Graham, Jogues, Karlsefni, M'Clintock, M'Clure, Nordenskiöld, and Joseph Burr Tyrrell) for whom entries are made have been misspelled, as well as a dozen or so more names occurring in the text. A few comments on what is said concerning some of the polar explorers may be of interest to readers of *Arctic*.

Those connected with the Northwest Passage are particularly troublesome for Mr. Langnas. Roald Amundsen, he claims, "was the second to manage the Northwest Passage", whereas most school children know that Amundsen was the first to negotiate the entire passage. He credits Sir William Parry, in 1819, with "discovering — after more than 300 years of vain attempts — the Northwest Passage". However, Sir Robert M'Clure, we read, in 1850-4, "completed discovery of the Northwest Passage". How he contrived to do this, when it was already discovered, is difficult to imagine. However, the honour and the reward were M'Clure's. Parry had not discovered a passage, although he did penetrate so far west as to leave the existence of such a route in no reasonable doubt. (Brown, R. N. R. Sir William Edward Parry. Arctic 12:104). Confusion is also abundant concerning the Northeast Passage. Amundsen, Mr. Langnas states, set out in 1918 to make this trip, "which failed after two years, as did another attempt in 1922-4". In fact, Amundsen arrived at Nome, Alaska in July 1920 after a successful passage on his initial attempt.

To say of Frederick A. Cook that "the kind of people who bought his old stock continued to believe that he had discovered the North Pole" is inaccurate journalism. In the sentences about Dr. Jean Charcot no mention whatever is made of his many visits to East Greenland and of his important work there. Dr. Hugo Eckener is described as using "heavier-than-air ships", whereas his fame rests

on his successful use of the lighter-than-air dirigible. Sir George Nares did not himself plant the British flag above 83°N., as Mr. Langnas says; this was done by a member of his expedition, Albert Markham, on a sledge trip. It appears from the dates given for the Soviet explorers Sergei and Vladimir Obruchev, that Vladimir was born two years later than his son — a "first" that even the Russians are unlikely to claim. In the sketch of Peary we read "a journey of the *Roosevelt*, 1905-6, brought him to 87°6′N." This triumph of navigation, if true, would have been a more remarkable feat than the attainment of the pole on foot; actually the ship wintered at Cape Sheridan (82°28′N.). In reading about Sir Hubert Wilkins one gathers that the Canadian Arctic Expedition lasted only from 1913 to 1914, rather than for four years longer.

The biography of Vilhjalmur Stefansson (both of whose names, it is pleasing to note, are spelled correctly) is especially confused. Stefansson's third expedition, as stated, did indeed reach "the northernmost point of Prince Patrick Island", but Stefansson also discovered three important islands beyond Prince Patrick Island on a journey to the northeast of about 300 miles. Following this we read that "his ship drifted for 11 months until he was rescued at Wrangel Island". Stefansson's ship, the *Karluk*, had foundered in the ice near Wrangel Island near the beginning of the expedition. Some men were rescued there, but Stefansson was not among them, being otherwise occupied. We read further that "in 1921 expeditions under his command explored Alaska and northeastern Siberia". The Wrangel Island expedition had nothing to do with exploration of Alaska. Its object was to claim for the British crown an island that was unoccupied and that was not then actively claimed by the Soviet Union. That "Stefansson now lives in New York City, where he has founded the Stefansson Library", will come as a suprise to many of us who had believed that this residence was terminated a decade ago and that his books now form a special collection of the Dartmouth College Library.

The dust-cover of this volume claims that, among other neglected men, "justice is done to a remarkable group of Jewish explorers. . . ." Everyone is pleased to see justice done, but in such a short dictionary it is questionable whether to mention many minor figures of Jewish extraction is a better justice than to omit several important explorers about whom the reader might expect information. Nor is the reader greatly edified to learn of Sven Hedin, a Swedish explorer, that in his retirement

> "He became a strong supporter of the Nazis, who paid him the compliment of ignoring his quarter-Jewish origin. In the middle of World War II, while Hitler was exterminating his fellow-Jews, he travelled to Munich to receive an honorary doctorate."

Many errors in the book could have been avoided by more careful proofreading, but there remain some important mistakes in fact. As it exists the book is too sketchy and too unreliable to be more than casually useful. *Webster's Biographical Dictionary* would be a better investment.

ALAN COOKE
Stefansson Collection

THE NEOLITHIC AGE IN EASTERN SIBERIA

By HENRY N. MICHAEL. *Philadelphia: American Philosophical Society. Transactions of the American Philosophical Society, New Series, Vol. 48, Part 2, 1958; 11¾ x 9½ inches; 108 pages, 7 maps, 100 text figures; paper. $2.50.*

This excellent monograph surveys all important Soviet archaeological research on the Neolithtic period of eastern Siberia to about the year 1950. Its geographical scope is confined to the official administrative territory of "Eastern Siberia". This includes the region east of the Yenisey River as far as the watershed between the Pacific and Arctic oceans, and from the Mongolian Republic north to the arctic coast. Hence it is not concerned with the Pacific

littoral region that comprises the Soviet Far East. It is a work that will have enduring value for American scholars who are interested not only in that specific region but also in the general problem of New World cultural origins, and most particularly for those who are unable to study the original reports in Russian.

The book begins with a clear statement of aims and limitations and then proceeds to a discussion of the various classification systems that have been used in the past to order the culture sequence in the Lake Baikal area. Next the cultural-stratigraphic analysis of the Angara River burials is carefully traced out, for this is fundamental for the Soviet chronology of the Siberian Neolithic and Bronze ages. The following chapters elaborate the significant characteristics of these burials and further treat problems of correlation and chronology. For additional refinement a separate chapter is devoted to an analysis of each of the major cultural periods, i.e., *Khina, Isakovo, Serovo,* and *Kitoi,* and finally there are area summations of the archaeological sequences in the middle Lena River valley, the lower Lena, and the arctic coast. With the exception of the arctic coast region there are adequate maps showing the location of all sites reviewed, the text is profusely illustrated with reproductions of artifact drawings that originally appeared in various Soviet publications, and several helpful summary tables are included.

In essence Michael's translation and distillation derive from the work of one man, A. P. Okladnikov. This is no cause for alarm, however, because Okladnikov on account of his extensive record is probably the most energetic archaeologist in his country today and he is also a scholar who works with skill, painstaking method and reasonable detachment when it comes to the interpretation of his data. Nevertheless, some balance of opinion is achieved through Michael's consideration of Soviet operations in areas that are peripheral to Eastern Siberia.

The one serious drawback of the monograph is no fault of the author's, for his main work was completed in 1954, some four years in advance of publication. Aside from several references to recent articles in Soviet journals, as well as pertinent reports of late date in American journals, it deals with Soviet materials that are prior to the 1950's. Since that time there has been a shift in certain key opinions and D. B. Shimkin, in a recent technical review of Michael (Am. Antiquity 24: 436-7, 1959), has listed the Soviet works of later date that should be consulted by the specialist who wishes to supplement Michael with more modern information. For example, whereas Okladnikov once asserted that the Palaeolithic Age in Eastern Siberia was an entirely autochthonous development, since the time of Michael's publication he has admitted that its early stages were of European origin. These initial events occurred in the Lake Baikal area and thence there was a steady diffusion northward down the valley of the Lena. In fact, the Russians stress that all early movements of man into the Eurasian arctic zone took place in a meridional direction from various southern centres of origin.

In this connection Michael lays some stress on the arctic zone of Eastern Siberia as the source from where the Palaeo-Eskimos of the Bering Strait region derived many of their significant culture traits. To some extent this energizes the moribund concept of an ancient circumpolar culture complex, but the problem of the origin of Eskimo culture certainly cannot be discussed satisfactorily without reference to the Soviet Far East and the Bering Strait coastal areas. Russian evidence does indeed point to circumpolar connections between Europe and western Siberia, and between Eastern Siberia and Bering Strait, but so far as is known now these concerned tundra cultures and appear to have been relatively late in time. Moreover, west of Bering Strait along the arctic coast the direction of the diffusion of Eskimo culture is not definitely known. As for the early components of Eskimo culture, the Russians believe, as Michael notes, that the roots of sea-mammal

economy are to be found well south of the arctic zone, along the littoral of eastern Asia.

Increased Soviet work in the Far East will ultimately serve to bridge the gap that now separates the archaeology of the Old World from that of the New. Probably not until then will the real potential of the prehistory of Eastern Siberia be appreciated. In the meantime Michael has served his colleagues well by making this extensive body of material available to them.

ELMER HARP, JR.

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Award of Institute Grants

The following have been awarded research grants by the Institute for 1960 from the Sir Frederick Banting Fund, The Ellsworth Foundation, funds of the Institute, and through contract with the United States Office of Naval Research:

BARNES, C. A. University of Washington, Seattle, Wash., U.S.A.
Review, analyse and interpret existing oceanographical data for the Arctic Ocean at the library of the University of Washington.

CARTER, J. C. H. McGill University, Montreal, P.Q., Canada.
Study of the ecology of the cod population of a brackish lake approximately 10 miles west of Nain, northern Labrador.

CHANCE, N. A. Harvard University School of Public Health, Boston, Mass., U.S.A.
Study the effect of winter conditions on the adjustment process of the Eskimos living at Barter Island, Alaska.

DUGDALE, R. C. Arctic Health Research Center, Anchorage, Alaska, U.S.A.
An investigation into the sources of phosphorus and nitrogen for lakes of subarctic and arctic Alaska, in southern Alaska.

FREEMAN, M. M. McGill University, Montreal, P.Q., Canada.
Studies of the ecology, physiology, and zoogeographical distribution of the genera *Gasterosteus* and *Pungitius* at Belcher Islands, N.W.T.

GEIST, O. W. University of Alaska, College, Alaska, U.S.A.
An examination of vertebrate Pleistocene fossil deposits in the region of the Meade and Utukok rivers, at Meade River, Alaska.

GORDON, M. S. University of California, Los Angeles, Calif., U.S.A.
Identification of antifreeze substances in the blood of fishes living in shallow arctic waters, at St. John's, Nfld.

GUBSER, N. J. Yale University, New Haven, Conn., U.S.A.
Comparative study of the intellectual culture of the Nunamiut Eskimos at Anaktuvuk Pass, Alaska.

HALL, E. R. University of Kansas, Lawrence, Kan., U.S.A.
To collect large series of mammals while the Russians are doing the same on Chukotsky Peninsula, U.S.S.R., at Seward Peninsula, Alaska.

HEDBERG, K. O. University of Uppsala, Uppsala, Sweden.
Preparation of microscopic slides of vascular plants from the Canadian Arctic, at the University of Uppsala, Sweden.

HIGASHI, A. Hokkaido University, Hokkaido, Japan.
Glaciological studies of the Mendenhall Glacier, at Juneau, Alaska, U.S.A.

HOBBIE, J. E. Indiana University, Bloomington, Ind., U.S.A.
Study of the primary production of an arctic lake, at Lake Peters, Alaska.

HOLMQUIST, C. University of Lund, Lund, Sweden.
Study of relatives of marine glacial relicts, at Friday Harbor and Barrow, Alaska.

HULTÉN, E. Riksmuseum, Stockholm, Sweden.
Field studies of certain groups in the arctic flora, at Barrow, Alaska.

HUME, J. D. Tufts University, Medford, Mass., U.S.A.
A quantitative study of the movement of clastic material in shallow, nearshore waters, at Barrow, Alaska.

HUSSEY, K. M. Iowa State University, Ames, Iowa, U.S.A.
Continuation of work on lake basins in the Barrow, Alaska, region.

JACOBSEN-MCGILL ARCTIC RESEARCH EXPEDITION, McGill University, Montreal, P.Q., Canada.
A study of the relationship between the atmosphere, the Axel Heiberg Island ice cap, and the adjacent marine environment with regard to heat budget, energy flow, and moisture transfer. A detailed investigation of the archaeology, biology, and geology of Axel Heiberg Island, at Axel Heiberg Island, N.W.T.

KREBS, C. J. University of British Columbia, Vancouver, B.C., Canada.
A study of lemmings, at Baker Lake, N.W.T.

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A study of the glacial geomorphology of the area north of Smith Arm, Great Bear Lake, N.W.T.

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Ecogeographical and systematic study of whitefish and char, in the Bering Sea drainage, Alaska.

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An ecological survey of the distribution of spiders in northern Alaska, at Barrow, Alaska.

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Studies of glacial geology and geomorphology, in the northern Torngat Mountains, Labrador.

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A complete study of the ecology and behaviour of the pomarine jaeger

population in the Barrow, Alaska, area.

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Hydrographical work in the Queen Elizabeth Islands to evaluate the geomorphic history, at Axel Heiberg Island.

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Study in economic geography of the fishing industry, in West Greenland and Copenhagen, Denmark.

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A study of the flora and vegetation of the Belcher Islands, N.W.T.

MORSE, S. A. McGill University, Montreal, P.Q., Canada.
Completion of doctoral thesis on bedrock geology of the Kiglapait-Manvers area, Labrador, at McGill University, Montreal, P.Q.

PARMELEE, D. F. Kansas State Teachers College, Emporia, Kan., U.S.A.
A study of the birds of Boothia Peninsula, at Shepherd Bay, N.W.T.

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A study of the energy metabolism to demonstrate adaptations to ecological circumstances, in Finland.

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A study of geomorphology and glacial geology in eastern Greenland, in the Sortehjorne area, Greenland.

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A study of the comparative ecology of lemmings and other microtines in northern Alaska, at Barrow, Alaska.

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A field study and mapping of the bedrock and Pleistocene geology of the Anaktuvuk Pass area, Brooks Range, Alaska.

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A study of the population of Atlantic salmon in the Whale River, Ungava.

PRESCOTT, G. W. Michigan State University, East Lansing, Mich., U.S.A.
A limnological and biological survey of Alaskan lakes, with special reference to their productivity, at Lake Peters, Alaska.

REED, B. Harvard University, Cambridge, Mass., U.S.A.
A study of the geology of an area in the Sadlerochit and Shubelik mountains, at Lake Peters, Alaska.

RITCHIE, J. C. University of Manitoba, Winnipeg, Man., Canada.
Compilation of a detailed vegetation map of Manitoba, at the University of Manitoba.

SATER, J. E. Arctic Institute of North America, Washington, D.C., U.S.A.
The collection of the material necessary for developing photogrammetric and photographic interpretation techniques for evaluating the surface morphology of sea ice, at Barrow, Alaska.

SCHUSTER, R. M. University of Massachusetts, Amherst, Mass., U.S.A.
Comparative taxonomic and ecological investigations of the Hepaticae of Greenland, at Thule, Greenland.

SHANKS, R. E. University of Tennessee, Knoxville, Tenn., U.S.A.
An investigation of the composition, structure, and productivity of the tundra vegetation of northern Alaska, at the University of Tennessee.

SMITH, N. G. Cornell University, Ithaca, N.Y., U.S.A.
A study of the distribution, variation, and interaction between Thayer's gull and American herring gull, at Southampton Island, N.W.T.

STEERE, W. C. New York Botanical Garden, New York, N.Y., U.S.A.
A critical field study and evaluation of arctic American mosses, at Barrow, Alaska.

TEDROW, J. C. F. Rutgers University, New Brunswick, N.J., U.S.A.
A study of the pedologic processes operating in the Arctic, at Barrow, Alaska.

WILCE, R. T. University of Massachusetts, Amherst, Mass., U.S.A.
Establishment of criteria for a marine flora of the higher latitudes in eastern Canada, at Thule, Greenland.

GEOGRAPHICAL NAMES IN THE CANADIAN NORTH

The Canadian Board on Geographical Names has adopted the following names and name changes for official use in the Northwest Territories and Yukon Territory. For convenience of reference the names are listed according to the maps on which they appear. The latitudes and longitudes given are approximate only.

Barrow Strait West 68 NW and 68 NE
(Adopted October 1, 1959)

Freemans Cove	75°07′N.	98°00′W.	
Intrepid Bay	74°57′	96°10′	
Longford Point	75°45′	103°15′	
Rapid Point	75°52′	97°35′	
Black Point	75°40′	97°24′	
Harding Point	75°09′	100°32′	
Royle Point	75°29′	97°03′	
Wood Island	75°26′	97°35′	
Bass Point	75°21′	97°50′	
Neal Islands	75°19′	97°30′	
Lacey Point	75°17′	97°51′	
Frazer Point	74°57′	98°35′	
Round Hill	75°10′	98°04′	
Dyke Acland Bay	75°00′	98°54′	not Acland Bay
McDougall Sound	75°10′	97°00′	not Macdougall Bay
Crozier Island	75°45′	96°35′	not Milne (Crozier) Island
Milne Island	75°36′	96°53′	
Scoresby Hills	75°53′	98°15′	not Scoresby Mountains

Keno Hill 105 M/14
(Adopted October 1, 1959)
Established names

Zone Creek	63°49′N.	135°28′W.	
Beliveau Creek	63°48′	135°29′	
Dawn Gulch	63°46′	135°14′	
Forty Pup (creek)	63°52′	135°23′	not 40 Pup (creek)

Auriol 115 A/12 E½
(Adopted October 1, 1959)

Mount Martha Black	60°40′N.	137°37′W.	not Mount Black

Kluane Lake 115 F E½ and G
(Adopted October 1, 1959)

Kletsan Hill	61°43′N.	140°58′W.	
Solomon Ridge	61°44′	140°50′	
Crag Mountain	61°25′	140°56′	
Mount Byron	61°27′	140°25′	
Icefield Ranges	61°13′	140°35′	
Yukon Plateau	61°45′	139°10′	
Steele Glacier	61°13′	140°05′	not Steele (Wolf Creek) Glacier
Altered application			
Koidern (locality)	61°59′	140°30′	
Name confirmation			
Mount Strickland	61°14′	140°40′	not Mount Black

Cape Smith 35 SW
(Adopted October 28, 1959)

Digger Lake	61°56′N.	79°34′W.	
Amulet Creek	61°55′	79°27′	

King William Island 67 SW and 67 SE
(Adopted October 28, 1959)

Enterprise Point	68°48′N.	103°19′W.	
Jenny Lind Bay	68°38′	101°47′	

Admiralty Inlet 48 SW and 48 SE
(Adopted October 28, 1959)

Mount Podolsky	72°40′N.	84°12′W.	
Kuhulu Lake	73°01′	84°20′	not Ecstall Lake

Rae Strait 57 SW and 57 SE
(Adopted October 28, 1959)

Wilkins Point	68°45′N.	93°42′W.	
Brands Point	68°49′	95°14′	

Coppermine 86 NW and 86 NE
(Adopted October 28, 1959)

McGregor Lake	66°52′N.	115°14′W.	
Speers Lake	66°59′	115°14′	
Stanbridge Lake	66°52′	115°04′	
Marceau Lake	66°26′	115°00′	
Muskox Lakes	66°34′	115°00′	not Musk-Ox Lakes

St. Elias 115 SW and 115 SE
(Adopted October 28, 1959)

Cattle Creek	61°35′N.	136°03′W.	
Triangulation Mountain	61°36′	136°22′	
McIntosh Lake	61°35′	136°58′	

Porcupine River 116 NW and 116 NE
(Adopted October 28, 1959)

Name confirmation			
Sunaghun Creek	67°25′N.	141°00′W.	not Sunagun Creek

Cambridge Bay 77 SW and 77 SE
(Adopted December 10, 1959)

Sinclair Creek	68°44′N.	108°57′W.	
Established name			
Cambridge Bay (post office)	69°07′	105°04′	

Simpson-Liard 95 SE
(Adopted December 10, 1959)

Spruce Creek	61°59′N.	121°46′W.	
Two Islands	62°00′	121°45′	

Sans Sault Rapids 106 H
(Adopted December 10, 1959)

Gibson Peak	65°48′N.	128°08′W.	
Beavertail Point	65°50′	128°53′	
Maida Creek	65°37′	128°16′	
Sammon's Creek	65°27′	128°13′	
Imperial Hills	65°28′	128°38′	not Imperial Mountains nor Imperial Range
Gibson Ridge	65°47′	128°03′	not Gibson Range
Axel Island	65°38′	128°37′	not Palm Beach Island nor Radium Charles Island
Name confirmation			
Norman Range	65°37′	127°55′	

Port Brabant 107 SW and 107 SE
(Adopted December 10, 1959)

Name confirmations			
Gulley Channel	68°28′N.	133°55′W.	
Taylor Channel	68°24′	134°45′	not Niligvik Channel
Little Moose Channel	68°37′	135°50′	not Rock Channel nor Little Moose River
Pete's Creek	69°12′	134°12′	not Petes Creek nor Pete Creek
Whitefish Station (locality)	69°23′	133°38′	not Whitefish (locality)
Altered applications			
Schooner Channel	68°22′	134°39′	
Holmes Creek	69°07′	134°21′	

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